Essays in Applied Labor Economics

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The Faculty of Economics, Business Administration and Information Technology of the University of Zurich hereby authorises the printing of this Doctoral Thesis, without thereby giving any opinion on the views contained therein.

Zürich, April 2nd 2008

the Dean: Prof. Dr. H.P. Wehrli

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Preface

Many ideas underlying this thesis were born in helpful discussions with my thesis adviser Josef Zweimüller and other members of his chair at the Institute for Empirical Research (IEW) where I had the luck of having an optimal research environment. Many other people also contributed to the success of this thesis and I would like to take the opportunity to thank them all.

I am grateful first and foremost to both Josef Zweimüller from the University of Zurich and Rafael Lalive from the University of Lausanne for their support. My debt to Josef Zweimüller goes back to my undergraduate studies where he taught me the fundamentals of the labor market and how to use theoretical models to understand the dynamics and relationships in a national economy. During my assistance he learned me who to use data and econometric techniques to answer important economic questions. My thanks also go to Rafael Lalive from whose knowledge and advice I have greatly profited during my whole studies. I am also grateful to Rainer Winkelmann, my second thesis advisor, not only for teaching me the basics in econometrics during my undergraduate studies, but also for his support, for providing me with helpful comments and the possibility to present my results in his research seminar. Further thanks go to my friends Reto Föllmi, Christian Heppenstrick, Michael Naef, Manuel Oechslin, Stefan Staubli, Olivier Steiger and Jean Philippe Wüllrich for their helpful discussions and comments during many research seminars or coffee breaks. Tanja Zehnder accompanied me since my undergraduate studies and I am glad that we are finishing our theses together. I am also grateful to Christoph Eisenegger, Sandro Favre, Jonathan Lorand, Markus and Sonja Knaden and Iris Steiger for proofreading parts of the manuscript.

I must record my especial appreciation to Andreas Kuhn not only for his endless patience answering my economic and econometric questions but also for his deep friendship which has helped me out of so many difficult situations. Special thanks go to Simon Büchi who always helped me up when I got lost in the data. And without a well functioning computer empirical research is impossible, therefore I have to thank Stefan Schmid for ensuring that my computer worked well throughout my whole assistance time.

I would like to thank my parents, Urs and Ursula, for their great support and for providing me with the opportunity to invest so much time in my studies. Finally I would like to thank Serena Simeone and Eric Périsset for their assistance whenever the going got rough.

During the hard time of writing this thesis, I also experienced light and hope with the birth of my nice Sereine – definitely proving that economics is not the center of life.

Zurich, 18 January 2008

CHAPTER 1

Introduction

> "Unter Arbeitskraft oder Arbeitsvermögen verstehen wir den Inbegriff der physischen und geistigen Fähigkeiten, die in der Leiblichkeit, der lebendigen Persönlichkeit eines Menschen existieren und die er in Bewegung setzt, so oft er Gebrauchswerte irgendeiner Art produziert."
>
> Karl Marx (1818-1883), "Das Kapital" (1872: p 148)

Most people in industrialized countries depend on the wage they earn in return for providing their physical or intellectual abilities. A substantial portion of individuals' available time is spent either as a part of the active workforce or in the educational system as a prestage of their future working life. The labor market circumscribes the complex interplay between the labor supply of employees and the labor demand of employers. It is the task of labor economists to try to understand the forces and dynamics underlying the labor market. Econometrics is one important tool which helps labor economists to comprehend the labor market. Yet in order to use econometric techniques the labor economist depends on the availability of data. This thesis is conceived as a contribution to the literature on empirical labor economics.

Employment is more than simply a contract between the employer and the employee setting the work conditions, the working hours or the wage. Beside the wage and the work time, employment involves several other positive and negative aspects. Many social interactions, which lead to new social contacts takes place at the workplace. Social exchanges at the workplace affect the behavior of employees and their job satisfaction (Seers *et al.*, 1995). The individual disadvantage of being unemployed compared with being employed is more than the monetary cost of receiving an unemployment benefit beneath ones earned income. Unemployment has an additional significant negative effect on individual happiness and life satisfaction (Winkelmann and Winkelmann, 1998; Lucas *et al.*, 2002). Beside these various positive aspects, employment also involves certain risks. Some risks directly affect the health of workers, such as the risk of an injury or occupational disease, other risks have indirect effects on individuals' well being, for example the risk of losing one's job.

This thesis will focus on two such risks, namely the risk of getting displaced due to a plant closure and the accident risk. More precisely, chapter 2 focuses on earnings losses of displaced workers and the question of whether the losses due to a plant closure depend on the size of the former employer and the business cycle. Chapter 3 and 4 focus on compensating wage differentials, for accident risk. Chapter 3 separates observed wages and risks into a worker and a firm specific wage and risk component. We thereby isolate the building block of the theory on compensation wage differentials, the firm specific wage and risk component. Chapter 4 provides estimates for the value of a statistical accident. We minimize the "ability bias" pointed out by Hwang *et al.* (1992) by focusing on workers of the lowest skill level.

1.1 Involuntary Job Losses

The termination of an employment relationship can have different reasons. From the point of view of the worker the termination may be voluntary or involuntary. A worker usually quits his job on his own initiative, because he has found a more desirable job. But there may be other reasons for a voluntary termination of employment, such as a move to another city or country, change of family situation or early retirement. A worker can suffer from being involuntarily laid-off because he is not productive enough. In such a case a worker can influence his risk of being laid-off through his own performance. But, even a worker with a high productivity can involuntarily lose his job for reasons beyond his control by being victim to a plant closure or a reorganization of his firm. In such a situation a worker has nearly no possibilities to prevent his displacement.

For a long time labor economists have been concerned with the issue of workers losing their jobs due to structural economic shocks, e.g. trade liberalization, government regulations or technological changes. When a firm goes bankrupt all workers lose their jobs, independent of their productivity, their position or their labor market history. Nevertheless, empirical studies show that some workers have a higher risk of losing their jobs due to a plant closure than others. For example in the U.S., Canada and the U.K. displacement rates tend to be higher for men than for women. In Denmark there is no difference between men and women and in Belgium women are more likely to get displaced than men. In most countries younger workers have a higher displacement rate than older workers (for a survey see Kuhn, 2002). Schwerdt (2005) points out that some workers leave the firm before the plant closure and he shows that these "early leavers" have higher re-employment chances than the workers who stay with their employer until bankruptcy. A large part of the economic literature on displaced workers focuses on workers' earnings losses after displacement. Using widely differing empirical methods and sources of data, many empirical studies agree that workers suffer lasting "scars" following a plant closure (e.g. Ruhm, 1991; Stevens, 1997; Farber, 1996). In Pennsylvania, for example, displaced workers suffer from long-term earnings losses of 25% per year. The pattern of the losses of workers displaced from firms with 50 to 500 employees is similar to the pattern of workers displaced from firms with 2,000 to 5,000 employees. The losses are larger for workers displaced from firms in the heavily unionized mining and construction industry. Also, the local labor market conditions affect the earnings losses of displaced workers. Workers' losses increase when the workers are displaced in regions with weaker labor markets conditions (for the Pennsylvanian case see Jacobson *et al.*, 1993). Gibbons and Katz (1991) shed light on another aspect of plant closure. The authors put forth that when the employer can decide which worker he wants to lay off, the market infers that these laid-off workers are of low productivity. In contrast to laid-off workers, those losing their jobs due to a plant closure are given no such negative label, because there is little to no connection between their productivity and the plant closure.

I argue that when the firm is small enough and the economic situation in which a displaced worker seeks employment is a boom period (where the plant closure is unlikely to be caused solely by aggravating market conditions) the labor market will infer that this displaced worker is less productive. Chapter 2 analyzes the labor market success of workers who are displaced in boom versus recession periods. Further, the empirical analysis contrasts workers from small firms versus large firms. Again, the idea is that displacement carries no information about workers' productivity in large firms but is a signal of low productivity for workers from small firms. This negative signal is worse when the plant closure occurs at the beginning of a longer boom period than at the beginning of a longer recession period. Results show that (i) the state of the business cycle has an important influence on the effect of displacement on labor market success and (ii) that this effect differs depending on the size of the former employer. In large firms, displaced workers suffer from larger earnings losses when displacement occurs during a recession as compared to during a boom; the opposite result holds for workers displaced from small firms.

1.2 Compensating Wage Differentials

More than 200 years ago, Adam Smith (1776) pointed out that "The wages of labour vary with the ease or hardship, the cleanliness or dirtiness, and the honourableness or dishonourableness of the employment". Since then labor economists have tried in different ways to quantify the relationship between such negative or positive job aspects and the wage the firm pays their workers. Empirical studies abound, which estimate compensating wage differentials for various workplace risks. For example in the U.S. agriculture sector, seasonal workers are compensated for their higher risk of unemployment by earning on average 15% more than permanent workers (Moretti, 2000). Hersch (1998) finds for the U.S. that a female worker, working on a job where she is exposed to an average level of job risk, receives a wage premium of \$400-\$563 per year as compensation for non fatal workplace risk. Using the estimated compensating wage differential for non fatal injury risk, the value of a statistical accident can easily be computed. The literature provides very different estimates of the value of a statistical accident, depending on various aspects, for example the estimation method, the underlying economic situation or the data quality. The estimates of the value of a statistical accident in the U.S. labor market range from \$20,000 to \$70,000. There is little evidence from outside the United States, but most of the European estimates falls within the U.S. range (see Viscusi and Aldy, 2003). The evidence on the quantitative impact of injury risks on the compensation of workers is still in debate. One main reason for the fact that many issues remain unsolved is the limited availability of data. While most authors have typically used longitudinal data sets for individual workers, information about the firm is mostly absent or extremely scarce in such data sets. However, firm information is of crucial importance from a theoretical point of view, as it is a characteristic of the workplace (rather than the worker) that leads to differences in wages.

Chapter 3 uses linked firm-worker data to study how workplace injury risks affect wages and the sorting of workers into different workplaces. Our analysis contributes to existing research by using data that reports, for the individual worker, incidences of workplace injuries together with the worker's complete earnings and employment history. Moreover, the data set allows us to link firms and workers, allowing us to identify the building blocks of the theory of compensating differentials: the wages and the risks that are specific to the firm. Furthermore, as we can also identify wages and risks attached to the worker, we can shed new light on the issue of how workers sort themselves across workplaces with different injury risks. To disentangle observed wages and workplace injuries into a component that is attached to the worker and a component that is particular to the firm, we borrow the econometric techniques developed by Abowd *et al.* (2002). The main results are: (i) The compensating differential (estimated from a regression of the firm-wage component on the firm-risk component) is roughly equal to the one obtained in the cross-sectional hedonic wage regression. (ii) We do not find evidence that more productive workers sort themselves into more secure jobs. (iii) However, our results indicate that high-risk workers select themselves into high-wage jobs suggesting that workers who are willing to take high injury risks are more likely to accept other workplace disamenities (and get compensated for them).

As a Swiss labor economist, I am especially motivated to contribute to the literature on the Swiss labor market. Chapter 4 deals with the compensation for non–fatal accident risk in Switzerland and presents empirical estimates of the value of a statistical injury by using data from the Swiss Wage Structure Survey (SWSS) and the Swiss Accident Insurance Fund (SAIF). The main problem of concern in our analysis is that there presumably is endogenous sorting of workers into jobs with different accident risks based on unobserved differences in productivity. Such kind of endogenous sorting leads to inconsistent estimates of the compensating wage differential. We approach this problem in two ways. First, having access to the number of accidents not only at the level of industries, but also within cells defined over industry$\times$skill–level of the jobs, allows us to estimate risk compensation within groups of workers defined over these cells. Second, we capitalize on the partial panel structure of the SWSS, which includes longitudinal information with respect to the employer. This principally allows for the empirical isolation of the wage component specific to the employer. This is of central importance since the theory of compensating wage differentials essentially relates to the firm–specific component of the wage, but not necessarily to the observed wage (or the wage component specific to the worker). These different approaches to identification yield in fact very different estimates of the value of a statistical injury. Our preferred estimate corresponds to about 40,000 Swiss Francs (per prevented injury per year), an estimate which actually lies within the range of estimates given by studies for the U.S. labor market. The results of our research on this topic also shed some light on the problem of endogenous sorting of workers.

CHAPTER 2

Effects of Firm Size and Business Cycle on Earnings Losses of Displaced Workers

"Capitalism without bankruptcy
is like Christianity without hell."

Frank Borman, retired NASA astronaut

2.1 Introduction

All of Switzerland was paralyzed by the grounding of Swissair, our national airline. We were shorn of the illusion that the white cross on the logo of our airline stood for stability and security. Nearly everyone around Zurich knows someone who was affected by the grounding of Swissair. Fortunately the government and the economy reacted, for Swiss circumstances, very rapidly by providing billions to build up a new airline out of the remains of Swissair. In doing so further negative consequences could be avoided, such as the potential demise of many intermediate firms dependent on Swissair.

For decades, economists and politicians alike have concerned themselves with the problem of workers loosing their jobs due to events beyond their control. Displaced workers suffer substantial earnings losses after a plant closure (e.g. Flaim and Sehgal, 1985; Hamermesh, 1989; Addison and Portugal, 1989; Ruhm, 1991). Because of the loss of firm-specific human capital, internal wages lie above alternative wages (Hamermesh, 1987). The earnings losses of displaced workers even hold in the long run (Jacobson *et al.*, 1993; Stevens, 1997). Contrary to workers who have been laid-off, a worker's displacement does not signal that he is less productive. Displaced workers have higher re-employment wages and shorter unemployment spells than laid-off workers (Gibbons and Katz, 1991). The idea behind this signaling hypothesis is that an individual worker is unlikely to have contributed to a plant closure, whereas being laid off is seen as a sign of low productivity.

This chapter proposes that the individual contribution to a plant closure and thus the signal about a worker's productivity is not necessarily insignificant and it depends on the size of the former employer and the business cycle. In general, this chapter argues first, that the probability that a plant closure is driven by a negative demand shock is larger in recessions than in booms and second, that firm performance is closely related to the effort exerted by an individual worker in small firms but not in large firms. Consider an economy with a asymmetric flow of information, where the workers' productivity is not observable to the employer. A new employer of a displaced worker would have no information about this workers' productivity besides the fact that his previous employer has closed down. Consider furthermore that this worker's previous firm was a one-man company that went bankrupt in an economic environment which was steadily improving. In this extreme example it seems natural that the individual contribution of this single worker to the plant closure cannot be ignored from the labor market. The question is: what does the new employer learn about the productivity of the worker, when his only information is that the worker's previous firm has gone bankrupt? This chapter argues that when a worker who was displaced from a small firm is looking for a new job in a boom period, the new employer infers that the worker is of low productivity. Workers displaced from a large firm or during a recession period do not carry such a negative signal.

The empirical analysis will contrast earnings losses for workers displaced in a boom from workers displaced in a recession and furthermore distinguish between workers displaced from small firms and large firms. This chapter will use workers who have been displaced from large firms as a benchmark, where individuals' contribution to the plant closure and thus the signal on workers productivity is almost zero. Because re-employment real wages are lower in a recession than in a boom (Dunlop, 1938; Tarshis, 1939; Baker *et al.*, 1994; Barlevy, 2001), workers getting displaced from large firms should suffer larger earnings losses when the plant closure occurs in a recession than in a boom. This argument also applies to small firms. In addition, workers who have been displaced from small firms also carry a negative signal because they would have had more influence on the success of the firm. One would expect that this negative signal should be worse in a boom than in a recession and thus induce larger earnings losses when the plant closure occurs in a boom. The net effect of the business cycle on future earnings for workers getting displaced from small firms is ambiguous.

The empirical analysis is based on a large administrative data set covering all Austrian private sector workers and their quarterly (un)-employment and earnings history from 1972 to 2001. A unique feature of the data is that by observing the size of the firm over time the exact date of each plant closure can be determined, this allowing one to compare earnings losses of workers displaced in a boom with workers displaced in a recession. Additionally, plant closing is observed at the firm level, which allows one to contrast large plant closure firms from small plant closure firms.

The main results of this chapter are, *first*, that workers getting displaced from large firms suffer larger earnings losses when they get displaced at the beginning of a recession period rather than at the beginning of a boom period. Separating the earnings effects into a wage and an unemployment effect results indicate that the probability of finding a new job after displacement is larger in a boom than in a recession. Wages are, however, not affected. Thus the large earnings losses in the recession period are driven by the accordingly larger fraction of unemployed displaced workers. These findings for workers displaced from large firms holds true during the entire post-displacement time period of six years. *Second*, workers who have been displaced from small firms experience significantly higher declines in earnings when they lose their job at the beginning of a boom period. This is in contrast to the findings for the large firms. Differences in the earnings losses between boom and recession are largely driven by the wages. Workers displaced from small firms suffer substantially larger declines in wages when the plant closure occurs at the beginning of a boom period. The probability of finding a new job after displacement is hardly affected by the business cycle. *Third*, there are no significant differences in pre-displacement earnings between boom and recession, neither for large firms nor for small firms. Thus the difference findings for the earnings losses during a boom versus a recession are not driven by ex-ante heterogeneity of workers and firms.

This chapter is organized as follows: Section 2.2 gives a short overview of related literature. Section 2.3 presents the data source and defines the sample of plant closure workers and the control sample of non plant closure workers respectively. Further, this section gives a short descriptive comparison of the earnings losses during a boom versus a recession, separately for workers displaced from large firms and workers displaced from small firms. Section 2.4 introduces different measures to capture the losses due to a plant closure and presents the statistical models used in this chapter. The results are shown and discussed in section 2.5. Section 2.6 concludes.

2.2 Related Literature

There are many studies analyzing long-term earnings losses of displaced workers. One of the most exhaustive studies is one from Jacobson *et al.* (1993). The authors use longitudinal data containing quarterly earnings histories for a large number of high tenure displaced and non-displaced workers from Pennsylvania, extending from 1974 to 1986. The authors find significant long-term earnings losses for the five years following the plant closure for high tenure workers. Further, they find that the earnings losses of displaced workers begin mounting before their displacement and depends only minimally on their age and sex. Jacobson *et al.* (1993) also find strong evidence that the earnings losses depend on the local labor market conditions. Workers displaced in a region with weak labor market conditions suffer higher earnings losses than workers displaced in a region with better labor market conditions. Stevens (1997) finds for the U.S. , in connection with a national data set based on a survey, that even six or more years after the plant closure displaced workers' earnings lie significantly below their expected levels. Further, she shows that a big part of these earnings losses can be explained by additional job losses following an initial displacement. Bender *et al.* (2002) compare, among other things, the wage profile of displaced workers after their job loss with the wages they earned right before the plant closure, using French and German data. In Germany, displaced workers earn significantly less in the three years after the plant closure but the earnings loss becomes insignificant after four years. In France, displaced workers earn even more in the first three years after the plant closure than directly before the plant closure. One explanation could be that high-wage workers find a new job earlier. The earnings difference becomes insignificant after four years as well. Thus, in contrast to the results from Jacobson *et al.* (1993) the earnings losses in France and Germany disappear in the long run. The reason for this may be the much more regulated labor markets in France and Germany, compared to in the United States.

Gibbons and Katz (1991) offer another approach by comparing the labor market success of plant closure workers with laid-off workers. They provide a theoretical and an empirical analysis of an asymmetric information model of layoffs. The idea behind their paper is that employers have private information concerning their employees' productivity and they are able to choose who to lay off. Accordingly, the market infers that laid-off workers are of low productivity and offers them lower wages in their next job. In other words, laid-off workers carry a negative signal concerning their productivity. The authors assume that workers displaced by a plant closure, in contrast, carry no signal concerning to their productivity, because the worker's individual contribution to the plant closure of a firm is negligible. Therefore the authors predict higher re-employment wages for plant closure workers than for laid-off worker. They confirm this prediction using the 1984-1986 Displaced Workers Survey (DWS) as a supplement of the Current Population Survey (CPS).

There is further literature which analysis either the effect of the business cycle or the effect of the size of the former employer. While not distinguishing between small firms and large firms, Nakamura (2004) develops a theoretical model in which displacement arises from a combination of selection and bad luck. The idea is that in every period some workers get laid off due to their low productivity, while the number of laid off workers due to bad luck would be higher during a recession. The proportion of workers who are laid off due to their low productivity would thus be larger during a boom. This implies that workers who get laid off in a recession are, on average, less adversely selected than those laid off during a boom. If the productivity of a worker is not observable to employers and the signaling effect varies over time, wage losses should also vary over time. Using the DWS as a supplement to the CPS she finds that the overall unemployment rate at the time of the job displacement has a significant positive effect on individuals' earnings losses. Farber (2005) also finds strong evidence that earnings losses of displaced workers are pro-cyclical to the business cycle. Also using the DWS and the CPS he finds earnings losses of about 7% during a recession as opposed to earnings losses of about 17% during a boom. Winter-Ebmer (2001) shows, using a large Austrian administrative data set, that larger firms not only pay higher wages but also offer more stable employment conditions. Krashinsky (2002) shows, using the National Longitudinal Survey of Youth, that the difference between earnings losses of laid off workers compared with those of displaced workers found by Gibbons and Katz (1991) becomes insignificant after controlling for the size of the former and the new employer.

Farber (1996), Kletzer (1998) and Abbring *et al.* (2002) investigate the effect of the business cycle on the displacement rate. For displaced workers, defined as permanent layoffs, the displacement rate is more or less anti-cyclical to the business cycle. The displacement rate, which includes only those workers displaced due to a plant closure, shows little variation over time.

2.3 Data and Descriptive Analysis

2.3.1 Data

The data source for the empirical analysis in this chapter is the "Austrian Social Security Database" (ASSD[1]), which contains detailed information about individuals' employment–unemployment– and earnings–history and several demographic characteristics such as age, gender, and broad occupation. The ASSD also contains detailed information on the employer, such as the size of the firm, the geographical location and the industry affiliation. The data set is unique in the following respects: first, one can exactly determine the date of a plant closure and thus precisely analyze workers' earnings losses over time. Second, one can observe plant closure at the firm level, allowing one to distinguish between small firms and large firms. This allows one to also control for firm specific characteristics in addition to individual worker's characteristics.

Individual earnings are available as earned income per workday. Earnings are measured in Euro and deflated by the consumer price index from 1986. For the purposes of this study, the 10th of February, May, August and November were used as quarter refer-

[1]For a detailed description of the ASSD see Kuhn and Ruf (2006)

ence dates[2]. Because the data was originally collected for Social Security purposes, there are also some disadvantages. The available earnings are right censored; about 15% of the workers have earnings above the upper censoring level, which varies over time. The data also does not contain any information about full or part time employment. However, the focus of this chapter is on income. The censoring may be problem. Yet, because workers suffer an earnings loss due to the plant closure, the fraction of censored wages will be larger in the group of non plant closure workers. This means the differences between the earnings of the plant closure (PC) workers and the non plant closure (NPC) workers will be attenuated due to the censored earnings.

The ASSD contains no direct information about plant closures. Yet, they can be constructed by analyzing the size of companies over time. A company is defined as a plant closure firm at the reference date t when two conditions are satisfied: (i) the firm disappears[3] between the reference dates t and $t+1$ and does not re-emerge during the following year[4] (ii) and less than 50% of the employees of a company find a new job with the same new employer. The latter criterion makes sure that company acquisitions are not erroneously classified as plant closures. In this chapter a worker counts as a PC worker at time t when he still works in the plant that goes bankrupt within the next quarter. That is, all PC workers will be displaced[5] within the next three months.

2.3.2 Sample Design

The aim of this chapter is to analyze the effect of job displacement on the future wage profile of displaced workers at two different points in the business cycle. Figure 2.1 shows the yearly growth rates of the real GDP (solid line) and the unemployment rates (dashed line), which are used as indicators for the business cycle.

As discussed in the introduction, the two points in time should be selected such that in one of them the economic situation steadily improves and in the other the economic conditions continuously deteriorate. In the statistical analyses, both a boom and a recession is made up of a period of eight consecutive quarters. The period 1987/1988 is followed by 5 years with relatively low unemployment rates (below 6%) and growth rates of the real GDP above 2%. This point in time, which is followed by a period where the economy expands relatively rapidly, is selected as the boom period. The period 1991/1992 is followed by six years with unemployment rates above 6%, and four years with growth rates of the real GDP below 2%[6]. This time period with relatively higher unemployment

[2]This setup implies that the data includes only information of the according reference dates.

[3]A firm disappears from the data, if it has no employees between t and $t+1$.

[4]This condition is set to one year, because the firm numbers were assigned anew after two years. That is, under the same firm number a completely new firm may emerge after two years.

[5]I will use plant closure and displacement as synonyms in this chapter.

[6]Expect for the year 1994, where the growth rate of the real GDP amounts to 2.4%.

Figure 2.1: Business cycle

rates and a more slowly expanding economy is selected as the recession period. Therefore by comparing the earnings losses between the years following the plant closure, a typical boom period is compared with a typical recession period. For each of these points in time a sample of workers is generated. Each of these samples contains all the workers who were employed at least at one quarter reference date in the according time period. The workers are split up in PC workers and NPC workers. The group of PC workers contains workers who were employed at least at one of the eight quarter reference dates in a plant closure firm. The group of NPC workers, used as the control group, contains workers who have never been employed in a plant closure firm during the same time period.

The data set includes the quarterly earning profile of each worker over six pre- and six post-displacement years for both points in time. To ensure that most of the workers exhibit earnings during this period and to avoid the problem of early retirement[7], the samples are restricted to prime-age workers aged 25-48. To compare the group of workers between the two points in time, the workers should have comparable costs of displacement. Workers who often change their firms and thus are used to being unemployed and looking for a new job would be less affected by an unexpected job loss than attached workers who want to keep their job. Furthermore, workers who leave a distressed firm before the

[7]In the ASSD we observe early retirement already for 55 years old worker.

Table 2.1: Number of displaced workers by the size of their company

Size of firm	Number of *PC* workers *boom*	*recession*
1 - 4	5'977 (5'203)	6'082 (5'337)
5 - 9	1'035 (472)	1'144 (509)
10 - 19	582 (135)	621 (155)
20 - 49	449 (55)	466 (55)
50 - 99	167 (8)	223 (12)
100 - 199	119 (2)	13 (1)
200 - 499	40 (1)	0 (0)
500 - 999	0 (0)	145 (1)
1000 +	0 (0)	591 (1)

Notes: The values in parentheses correspond to the number of firms.
Source: Own calculations based on ASSD.

bankruptcy is apparent, have different earning profiles than worker who stay until the end (Bowlus and Vilhuber, 2002; Schwerdt, 2005). Accordingly, the sample is restricted to attached workers, with at least two years of tenure. An additional advantage of this restriction is that workers who are only seasonally unemployed are dropped too.[8] In addition, all observations without information about industry or region were excluded from the sample.

Table 2.1 shows the number of PC workers who fullfill the restrictions discussed above, listed according to the size of their firms. The numbers in parentheses correspond to the number of the involved firms.[9] Up to the size of firms with 100 employees the number of PC workers does not differ much between boom and recession. But with the bigger firms come larger differences. In order to have as few differences as possible between boom and recession, firms with more than 100 employees were excluded. Further, the employees should have comparable information about the future financial problems of their company before the plant closure. The smaller a company the better would be the employees' information about the imminent bankruptcy of their employer. Thus, the large group of firms with less than five employees is excluded from the samples. An other reason why firms with less than five employees were excluded, is that it is difficult to identify a plant closure for tiny firms. To analyze different earnings losses depend on the size of the

[8]In Austria there are a lot of seasonal workers. For example, in the constructing industry many workers are disbanding in the unemployment during the winter because of the smaller volume of orders. For more detailed information about seasonal workers and the restriction of two years of tenure see Ruf (2004).

[9]At first sight it's confusing that in the firm in the group of 500 - 999 employees only 145 workers were employed. At second sight, it gets clear that only 145 workers of this firm are aged 25-48 and have at least two years of tenure.

former employer, this article distinguishes between small firms, which employ between five and nine workers, and large firms, which employ between ten and one hundred workers.

For the empirical analysis a 2% random sample of the NPC workers and all the PC workers were used. After imposing all the restrictions mentioned in this section there remain for the boom period: 1,035 (9,596) PC (NPC) workers in the sample of small firms and 1,167 (37,933) in the sample of large firms. For the recession period: 1,144 (9,640) in the sample of small firms and 1,191 (39,198) in the sample of large firms

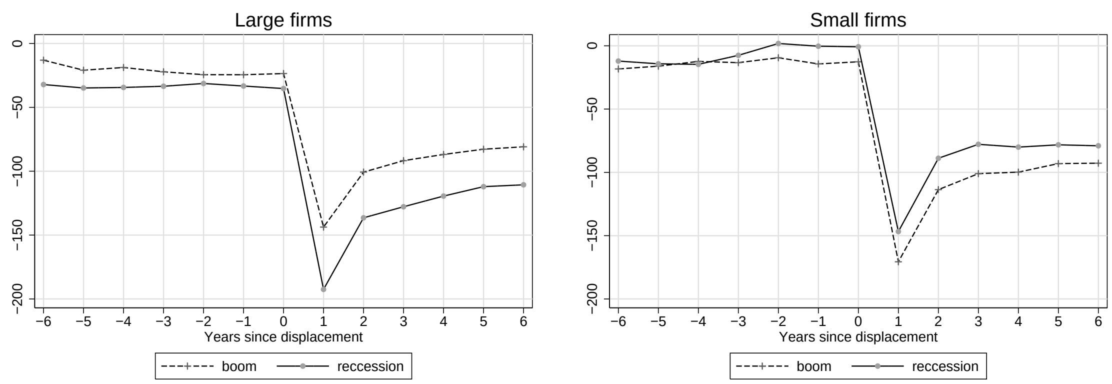
2.3.3 Descriptive Analysis

First, I calculate the average earning differences of the PC workers minus the NPC workers for the six pre- and the six post-displacement years. The left subfigure of figure 2.2 shows the average differences for the large firms and the right column those for the small firms. The differences in earnings are computed on the vertical axis and the years since displacement on the horizontal axis. The solid lines correspond to the differences in the boom period whereas the dashed lines correspond to those of the recession period.

In both points in time, workers displaced from large firms earn on average a little less than the NPC workers during the six pre-displacement years. The differences are slightly larger during a recession than during a boom. The PC workers suffer a large earnings loss during the first years; subsequently the difference in earnings to the NPC workers gets smaller over time. Workers displaced from large firms suffer larger earnings losses over the whole post-displacement period when the plant closure occurs during a recession than during a boom.

For workers displaced from small firms the differences in earnings between the PC workers and the NPC workers is almost zero during the six pre-displacement years. The path of the earnings losses in the post-displacement years appears similar to the results

Figure 2.2: Average differences in earnings over time



Source: Own calculations based on ASSD.

for the large firms. But in contrast to workers displaced from large firms, those displaced from small firms suffer larger earnings losses over the whole post-displacement period when the plant closure occurs during a boom than during a recession. Summed up, during a recession workers suffer larger earnings losses, when they are displaced from large firms, whereas during a boom the earnings losses are larger for workers displaced from small firms.

Table 2.2 shows the average characteristics of workers and firms, for each group correspondingly. There are only small differences between the average PC worker and the average NPC worker. In all the samples PC workers are on average older than the NPC workers; nevertheless PC workers are less attached to their firms. In the sample of small firms, seasonal firms[10] have a higher probability to go bankrupt at the beginning of a boom period than non seasonal firms, whereas during a recession there is almost no such difference. It is unclear, however, whether the ex ante heterogeneity of workers and firms of the different groups are responsible for the differences found in this section. Therefore, in the next section, regressions are made which control for these ex ante heterogeneities.

2.4 Identification and Statistical Models

The aim of this chapter is to determine if earnings losses due to a plant closure depend on the size of the former employer and the business cycle. To analyze this question the empirical part distinguishes four groups of workers: those who are (i) employed in large firms during a recession, (ii) employed in large firms during a boom, (iii) employed in small firms during a recession and (iv) employed in small firms during a boom. Subsection 2.4.1 discusses how earnings losses are identified. Subsection 2.4.2 presents separate regressions, which, on the one hand, are used to estimate the earnings losses for the four groups and, on the other hand, to estimate the differences in the earnings losses between boom and recession periods, for large and small firms respectively.

2.4.1 Identification of the Losses due to Plant Closure

A decline in earnings may result from a combined effect of a decline in wages and a larger probability to be unemployed. This means that a lower expected income for a group of workers can be the result of a lower average wage and/or more unemployed workers with zero income within this group. This can be written as follows:

$$E_{gt}(earning_i) = E_{gt}(wage_i | U_i = 0) \cdot E_{gt}(U_i = 0).$$

Here, $earning_i$ corresponds to the earning of individual i in a specific year before or after displacement. Thus, $E_{gt}(earning_i)$ corresponds to the expected earnings of a group of

[10]Seasonal firms includes the building industry and the tourist sector.

Table 2.2: Descriptive statistics

	SMALL FIRMS				LARGE FIRMS			
	boom		*recession*		*boom*		*recession*	
	PC	*NPC*	*PC*	*NPC*	*PC*	*NPC*	*PC*	*NPC*
Wage	446.08	458.71	474.12	480.01	498.44	521.97	504.58	552.04
	(200.78)	(193.23)	(204.00)	(204.39)	(198.13)	(190.93)	(199.95)	(204.35)
Log wage	5.98	6.04	6.05	6.07	6.11	6.18	6.07	6.22
	(0.48)	(0.48)	(0.48)	(0.48)	(0.45)	(0.44)	(0.44)	(0.43)
Age	37.31	36.80	36.72	36.31	37.38	36.96	36.82	36.47
	(6.98)	(7.05)	(7.05)	(6.93)	(7.01)	(7.06)	(7.04)	(6.93)
Female	0.55	0.51	0.50	0.51	0.45	0.40	0.47	0.39
	(0.49)	(0.49)	(0.49)	(0.49)	(0.49)	(0.48)	(0.49)	(0.48)
Blue collar	0.42	0.40	0.41	0.42	0.48	0.44	0.51	0.45
	(0.49)	0.49)	(0.49)	(0.49)	(0.49)	(0.49)	(0.49)	(0.49)
Tenure	7.71	8.00	7.29	7.76	8.33	8.44	7.87	8.34
	(4.62)	(4.49)	(5.24)	(5.11)	(4.68)	(4.63)	(5.61)	(5.41)
Size of the firm	6.63	6.79	6.60	6.82	25.94	40.05	28.66	39.70
	(1.38)	(1.39)	(1.38)	(1.40)	(19.16)	(25.35)	(21.53)	(25.26)
Manufacturing	0.32	0.27	0.33	0.25	0.47	0.34	0.51	0.32
	(0.46)	(0.44)	(0.47)	(0.43)	(0.49)	(0.47)	(0.49)	(0.46)
Seasonal industry	0.22	0.14	0.16	0.15	0.17	0.13	0.21	0.14
	(0.41)	(0.34)	(0.37)	(0.35)	(0.37)	(0.33)	(0.41)	(0.34)
Other industries	0.45	0.59	0.50	0.60	0.36	0.53	0.28	0.54
	(0.49)	(0.49)	(0.50)	(0.49)	(0.48)	(0.49)	(0.44)	(0.49)
Number of workers	1'035	9'596	1'144	9'640	1'167	37'933	1'191	37'189
Number of firms	472	8'360	509	17'873	198	8'495	222	19'267

Note: Standard errors in parantheses. Source: Own calculations based on ASSD.

workers g at time t. This expectation can be written as the product of the expected wage of all employed workers of group g at time t $E_{gt}(wage_i|U_i = 0)$ multiplied with the expected probability to be employed within this group of workers $E_{gt}(U_i = 0)$. From here onwards, the effect of the plant closure on the probability to be unemployed will be called the unemployment probability (UP) effect and the effect on the wage, the wage effect. For a change in expected wages there are two sources; a selection effect and a net wage effect. The selection effect occurs because more productive workers with higher average wages find a new job faster than less productive workers with lower wages. The net wage effect reflects changes in the wage of a specific worker, $wage_i$, relative to his average wage of the two years prior to displacement $\overline{wage}_i$[11]. This can be written as follows:

$$E_{gt}(wage_i|U_i = 0) = E_{gt}(\overline{wage}_i|U_i = 0) + E_{gt}(wage_i - \overline{wage}_i|U_i = 0).$$

The variable $\overline{wage}_i$ can be used as a proxy of productivity for this worker (which is not observed in the ASSD). Changes in $E_{gt}(\overline{wage}_i|U_i = 0)$ are only induced by variations in the composition of employed workers. This is termed the selection effect. $E_{gt}(wage_i - \overline{wage}_i|U_i = 0)$ captures the net wage decline for employed workers. This is the net wage effect, which is unaffected by time invariant unobserved individual characteristics.

2.4.2 Statistical Models

It is not clear if the ex-ante heterogeneity between workers and their firms is responsible for the observed differences in average earnings discussed in the descriptive part. To take this into account, one can estimate the following linear regression model for each group of workers with:

$$W_{it} = \alpha + (pc_i \cdot y_t)'\beta + y_t'\gamma + x_i'\delta + f_i'\eta + q_i'\theta + \epsilon_{it} \qquad (2.1)$$

where W_{it} corresponds to one of the variables discussed above. (i.e., $earnings_{it}$, $wages_{it}$, UP_{it}, $\overline{wage}_i$ or $(wage_{it} - \overline{wage}_i)$). Earnings and wages are deflated by the average[12]. pc_i is a dummy variable taking on the value one for PC workers and zero otherwise. y_t is a vector of dummy variables representing the years since displacement (from six years before to six years after displacement). The vector x_{it} includes individual characteristics like age, age squared, gender, tenure, tenure squared and a dummy variable for blue collar workers in order to control for the ex-ante heterogeneity of workers. The vector f_{it} contains firm related characteristics like industry (two digit), size, size squared and the location of the firm (nine different states, "Bundesländer") in order to control for the

[11]During this two years all the individuals are employed, because a tenure of at least two years is required to be included in the sample.

[12]This means that the individual earnings/wages are divided by the average earnings/wages of all workers at the reference date (i.e. year = 0).

ex-ante heterogeneity of firms. The vector q_{it} includes three dummy variables for each quarter of the reference date where the according workers are defined as PC workers or NPC workers; the first quarter is the base quarter and thus not included in the regression. Finally, ϵ_{it} is an error term assumed to be independent of observed characteristics. The time index t identifies the year since displacement and the index i refers to individuals. The vector of primary interest is β, which captures the differences in the variable of interest between PC workers and NPC workers over the observation period.

The focus of this chapter lies in the differences in the earnings losses between boom and recession periods. To directly estimate this difference in difference (DiD) parameter and to test whether they differ significantly from zero, the samples from boom and recession periods are merged together and additional regressions are run for each variable of interest, separately for the sample of small and large firms. The regression has the following form:

$$\begin{aligned} W_{it} &= \alpha + (R_i \cdot pc_i \cdot y_t)'\beta^R + (R_i \cdot y_t)'\gamma^R + (R_i \cdot x_i)'\delta^R + (R_i \cdot f_i)'\eta^R \\ &\quad +(R_i \cdot q_i)'\theta^R + (pc_i \cdot y_t)'\beta + y_t'\gamma + x_i'\delta + f_i'\eta + q_i'\theta + \epsilon_{it} \end{aligned} \tag{2.2}$$

R_i is a dummy variable taking on the value one for the recession and the value zero for the boom. The DID parameter vector β^R captures the additional earnings losses for workers displaced during a recession compared with workers displaced during a boom.

2.5 Empirical Findings

The descriptive part showed that workers displaced from large firms suffer on average a larger earnings loss when the displacement occurs during a recession than when it occurs during a boom. The opposite is true for workers displaced from small firms. However, these results may be driven by the ex-ante heterogeneity of workers or firms. The advantage of the regression framework presented above is that it explicitly controls for such heterogeneity among workers and firms. The estimation results for β^R (and the corresponding t-values) are shown in tables 2.3 and 2.4[13].

In order to better understand the DiD estimates, estimated first differences of all outcome variables ($\hat{\beta}$ in the regression given by specification 2.1) are presented as well. Figures 2.3 and 2.4 display the results graphically. The differences of each outcome variable are displayed on the vertical axis and the years since displacement on the horizontal axis. The year since displacement is marked with an asterisk, when the difference between boom and recession is statistically significant on the 5% level. The left column of the subfigures shows the estimated differences for large firms and the right column those for small firms. The solid line corresponds to the estimated differences from the recession period and the dashed line to the estimated differences from the boom period. Each difference

[13] Full regression results are available from the author on request.

Figure 2.3: Conditional differences over time

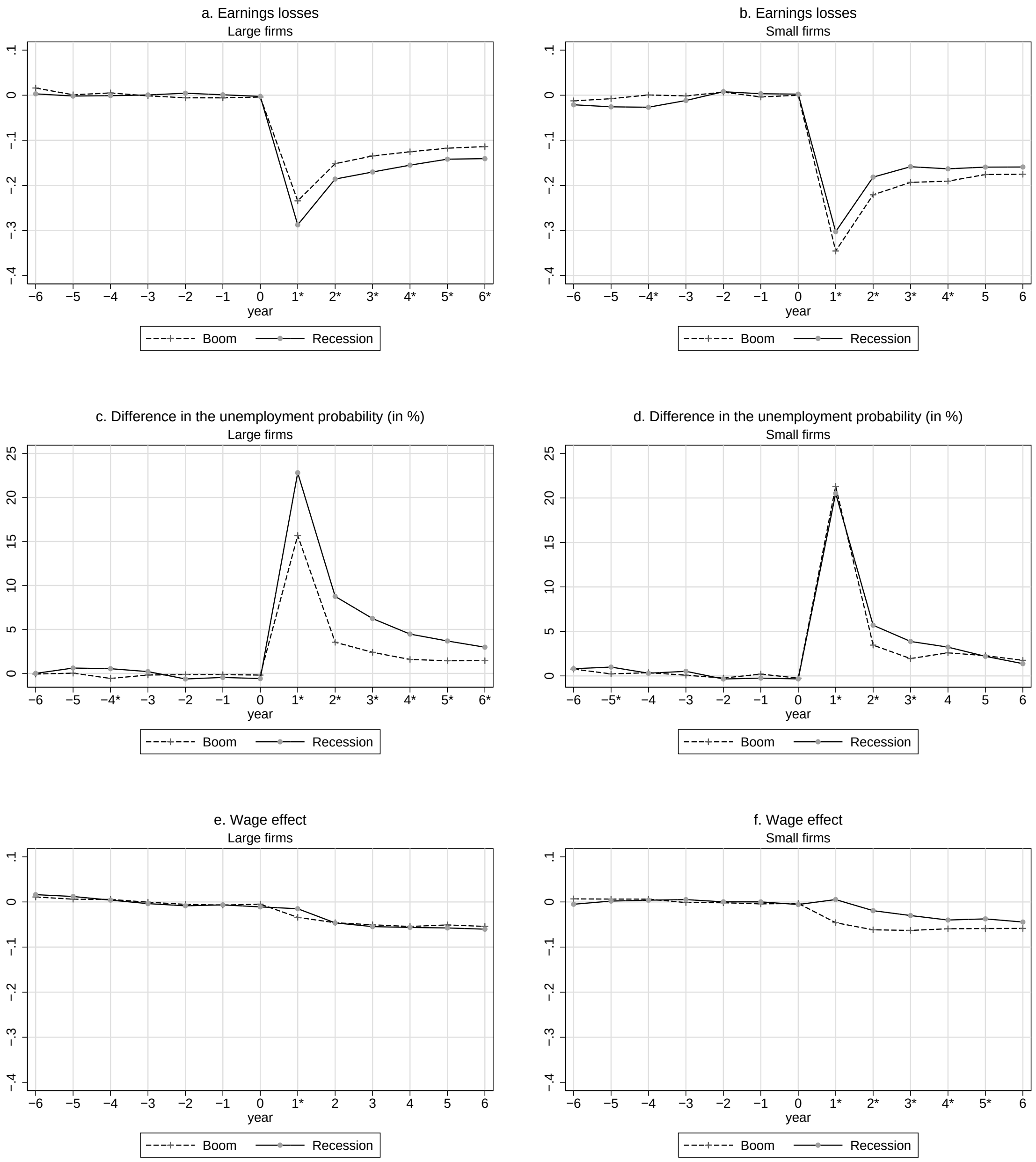


Table 2.3: Difference in differences: earnings, UP and wages

Sample	Large firms			Small firms		
Dep. Variable	Earnings	UP	Wages	Earnings	UP	Wages
Year -6	-0.013	0.07 %	0.005	-0.009	0.00 %	-0.013
	(1.62)	(0.23)	(0.78)	(0.87)	(0.01)	(1.51)
Year -5	-0.003	0.58 %	0.006	-0.018	0.73 %	-0.005
	(0.36)	(1.89)	(0.90)	(1.79)	(2.05)*	(0.63)
Year -4	-0.006	1.10 %	-0.002	-0.027	-0.09 %	-0.003
	(0.77)	(3.60)**	(0.33)	(2.68)**	(0.25)	(0.38)
Year -3	0.002	0.38 %	-0.004	-0.010	0.40 %	0.006
	(0.24)	(1.25)	(0.63)	(1.01)	(1.12)	(0.73)
Year -2	0.010	-0.49 %	-0.004	0.001	-0.14 %	0.001
	(1.27)	(1.61)	(0.65)	(0.08)	(0.39)	(0.13)
Year -1	0.007	-0.32 %	-0.000	0.007	-0.48 %	0.003
	(0.83)	(1.05)	(0.01)	(0.69)	(1.33)	(0.44)
Reference date	0.001	-0.40 %	-0.007	0.002	-0.11 %	-0.003
	(0.08)	(0.66)	(0.58)	(0.11)	(0.16)	(0.23)
Year 1	-0.053	7.12 %	0.018	0.043	-0.82 %	0.050
	(6.61)**	(23.33)**	(2.69)**	(4.19)**	(2.29)*	(5.32)**
Year 2	-0.034	5.21 %	-0.001	0.039	2.20 %	0.042
	(4.25)**	(17.05)**	(0.13)	(3.84)**	(6.14)**	(4.72)**
Year 3	-0.036	3.83 %	-0.004	0.034	1.89 %	0.032
	(4.43)**	(12.55)**	(0.69)	(3.39)**	(5.27)**	(3.63)**
Year 4	-0.030	2.88 %	-0.003	0.027	0.60 %	0.018
	(3.71)**	(9.42)**	(0.41)	(2.67)**	(1.69)	(2.05)*
Year 5	-0.024	2.24 %	-0.007	0.016	-0.11 %	0.020
	(3.02)**	(7.34)**	(1.11)	(1.60)	(0.31)	(2.23)*
Year 6	-0.027	1.53 %	-0.007	0.016	-0.41 %	0.013
	(3.30)**	(5.00)**	(1.01)	(1.54)	(1.14)	(1.41)
Observations	4113911	4120802	3704037	1094892	1096914	956995
R^2	0.23	0.02	0.31	0.19	0.03	0.28

Notes: **, * denotes significance at the 1% , 5% level respectively. T-values in parentheses. UP is the unemployment probability times 100.
Source: Own calculations based on ASSD.

between the boom values and the recession values in the subfigures of figure 2.3 and 2.4 corresponds to a DiD estimate in table 2.3 and 2.4.

2.5.1 Main Results

The upper panel of figure 2.3 shows the estimated earnings losses of displaced workers. During the first year after plant closure, displaced workers suffer a strong decline in earnings in the range of 23% to 34%, depending on the size of the former employer and the business cycle. For all groups of workers the earnings losses get smaller over time but are still substantially higher than 10% even six years after displacement. The most striking difference between workers displaced from small firms (subfigure b) and workers displaced from large firms (subfigure a) is that for the former the earnings losses are higher during a boom than during a recession, whereas for workers displaced from large firms the results are the opposite.

As column one of table 2.3 shows, workers displaced from large firms face an earnings loss during the first post-displacement year which is 5.3 percentages points (p.p.) higher if the displacement occurs during a recession rather than during a boom. Six years after displacement the DiD still amounts to -2.7 p.p.; in other words, workers displaced from large firms in a recession suffer higher earnings losses than those displaced from large firms in a boom even in the long run. Workers displaced from small firms suffer earnings losses which are 4.3 p.p larger when the plant closure occurs during a boom rather than during a recession (column four of table 2.3). These DiD estimates decrease over time to 2.7 p.p four years after the displacement. Five years after the displacement the difference between boom and recession becomes insignificant. This provides first evidence supporting the hypothesis that workers displaced from small firms during a boom are treated differently by the labor market and suffer the highest earnings losses. One explanation for this finding could be that workers displaced from small firms and seeking new jobs during a boom carry a negative signal with them due to their displacement.

Interestingly, the results for Austria for the post-displacement years are in line with the findings of Jacobson *et al.* (1993) who did similar research with data from Pennsylvanian. For all workers the DiD estimates in earnings are insignificant for the six pre-displacement years.[14] This implies that workers have no ex-ante differences in earnings after controlling for individual-specific and firm-specific characteristics, ruling out a selection explanation.

To identify the source of variation in the earnings losses, I split them up into the UP effect and the wage effect, accounting for the fact that a decline in earnings for one specific group of workers can arise either from a higher fraction of unemployed workers with zero work income or just from lower wages. Subfigure *c* and *d* of figure 2.3 show that

[14]The only exception is the significant DiD of -2.7% four years before the displacement for workers displaced from small firms.

one year after displacement PC workers are faced with a 15% to 22% lower probability to be employed than NPC workers, depending on the size of the former employer and the business cycle. This is not surprising because, per definition, all PC workers lose their jobs. The difference in the UP between PC and NPC workers is more than halved during the second year and then decreases slowly thereafter to about 2.5 % six years after displacement. Hence, a big part of the large earnings losses during the first post-displacement year can be explained by the large fraction of PC workers that do not find a new job immediately.

Column two of table 2.3 shows that workers displaced from large firms have a 7 p.p. lower probability to find a new job during the first post-displacement year if the plant closure occurs during a recession rather than during a boom. This positive DiD in the UP decreases over time to 1.5 p.p. six years after the displacement. In addition to the lower probability of finding a new job during the first year, workers displaced from large firms during a recession earn 1.8 p.p lower wages than those displaced in a boom (column three of table 2.3). The positive DiD in wages becomes insignificant after the second year. Thus the higher earnings losses for workers displaced from large firms in a recession as opposed to boom are largely driven by the higher fraction of unemployed workers with zero earnings.

For workers displaced from small firms, things are different, as shown in columns five and six of table 2.3. In the first post-displacement year workers displaced in a recession have better chances of finding a new job than workers displaced in a boom and thus the according DiD is negative (column five). From the second to the fourth year after displacement workers displaced in a boom have better chances of finding a new job than those displaced during a recession. During the following two years the DiD estimates in the UP becomes insignificant. Therefore, the larger earnings loss during the first post-displacement year for workers displaced in a boom rather than during a recession is due to a combined effect of the higher probability of being unemployed and lower wages. In following three years the significantly higher earnings losses are mainly driven by the large wage decline, because the positive DiD in the UP imposes ceteris paribus larger earnings losses during a recession.

The separation of the earnings losses into a wage effect and an UP effect shows that, the fact that workers displaced from small firms suffer higher earnings losses during a boom instead of during a recession is largely due to the large reduction in wage. The same method shows that the main driving factor of the opposite phenomenon, that workers displaced from large firms suffer higher earnings losses during a recession as opposed to during a boom, is the larger fraction of unemployed workers with zero earnings.

Next, I will present the results from the separation of the wage effect into the selection and the net wage effect. The selection effect picks up differences in the composition of employed workers and sheds light on two different aspects. First, comparing the selection

Figure 2.4: Conditional differences over time

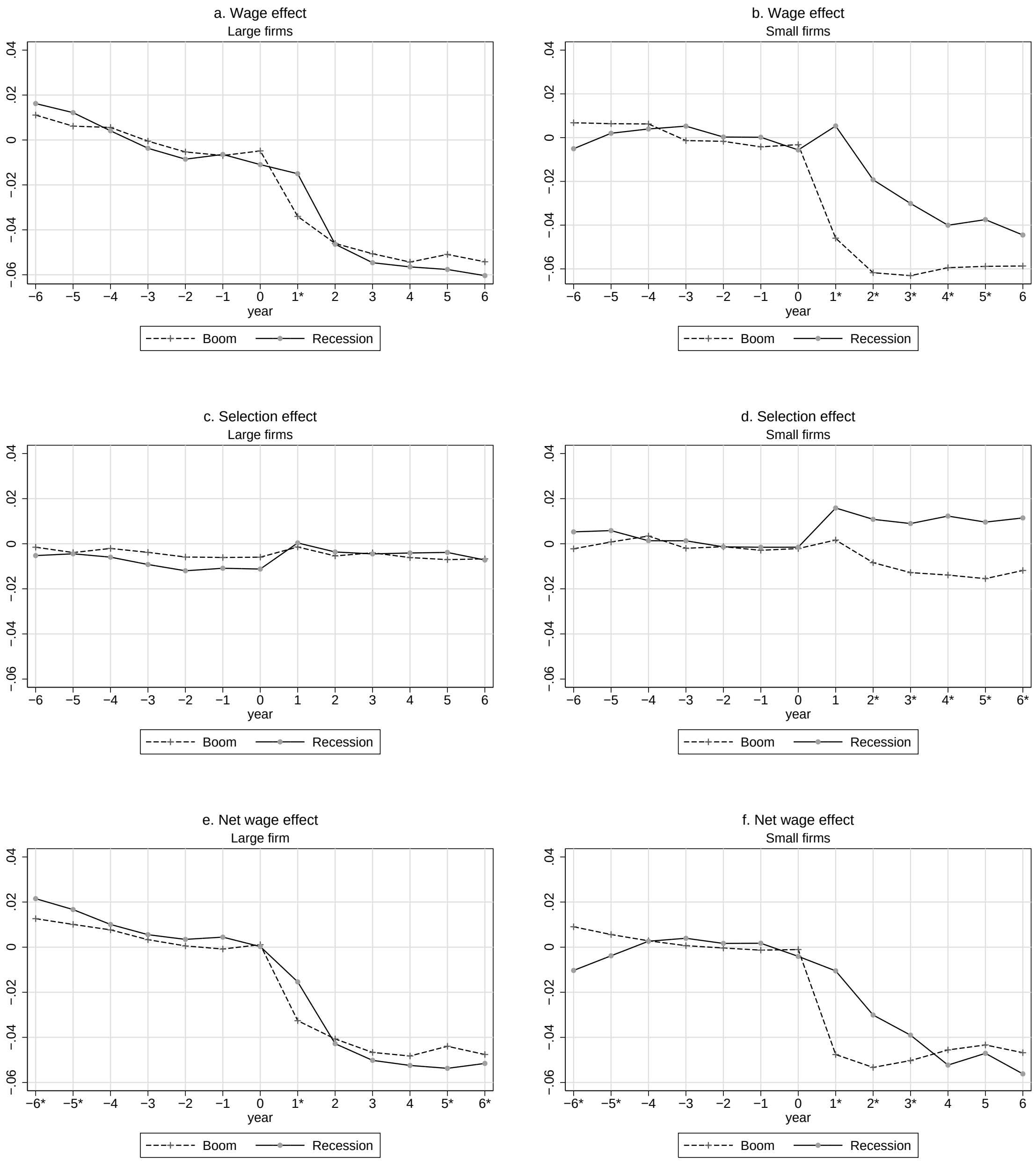


Table 2.4: Difference in differences: wages, selection effect, net wage effect

Sample	Large firms			Small firms		
Dep. Variable	Wages	SE^a	NWE^b	Wages	SE^a	NWE^b
Year -6	0.005	-0.004	0.009	-0.013	0.007	-0.019
	(0.78)	(0.64)	(2.63)**	(1.51)	(0.85)	(4.32)**
Year -5	0.006	-0.001	0.006	-0.005	0.004	-0.010
	(0.90)	(0.16)	(2.01)*	(0.63)	(0.55)	(2.15)*
Year -4	-0.002	-0.004	0.002	-0.003	-0.003	-0.000
	(0.33)	(0.76)	(0.73)	(0.38)	(0.36)	(0.07)
Year -3	-0.004	-0.006	0.002	0.006	0.003	0.003
	(0.63)	(1.06)	(0.69)	(0.73)	(0.34)	(0.75)
Year -2	-0.004	-0.007	0.003	0.001	-0.001	0.002
	(0.65)	(1.22)	(0.93)	(0.13)	(0.12)	(0.46)
Year -1	-0.000	-0.005	0.005	0.003	0.000	0.003
	(0.01)	(0.97)	(1.71)	(0.44)	(0.07)	(0.71)
Reference date	-0.007	-0.006	-0.001	-0.003	-0.000	-0.003
	(0.58)	(0.53)	(0.16)	(0.23)	(0.02)	(0.39)
Year 1	0.018	0.001	0.017	0.050	0.013	0.037
	(2.69)**	(0.22)	(4.73)**	(5.32)**	(1.49)	(7.29)**
Year 2	-0.001	0.001	-0.002	0.042	0.018	0.023
	(0.13)	(0.22)	(0.65)	(4.72)**	(2.22)*	(4.90)**
Year 3	-0.004	-0.001	-0.004	0.032	0.021	0.011
	(0.69)	(0.15)	(1.06)	(3.63)**	(2.51)*	(2.36)*
Year 4	-0.003	0.002	-0.004	0.018	0.025	-0.007
	(0.41)	(0.27)	(1.25)	(2.05)*	(2.99)**	(1.43)
Year 5	-0.007	0.003	-0.010	0.020	0.024	-0.004
	(1.11)	(0.44)	(2.87)**	(2.23)*	(2.81)**	(0.80)
Year 6	-0.007	-0.001	-0.004	0.013	0.022	-0.010
	(1.01)	(0.16)	(1.15)	(1.41)	(2.58)**	(1.91)
Observations	3704037	3704037	3704037	956995	956995	956995
R^2	0.32	0.31	0.29	0.28	0.27	0.23

Notes: **, * denotes significance at the 1% and 5% level respectively. T-values in parentheses. UP is the unemployment probability * 100. (a) SE reports the selection effect given by $\overline{wage}$. (b) NWE reports the net wage effect given by $(wage_i - \overline{wage}_i)$
Source: Own calculations based on ASSD.

effect before and after the plant closure for one specific group of workers helps to understand if the composition of employed workers within this group is affected by the plant closure. Second, the estimated DiD between the selection effect during a boom versus during a recession helps to better understand the DiD estimates found for the wage effect. A positive DiD of the selection effect indicates that the fraction of re-employed high wage workers is larger during a recession than during a boom. The net wage effect at time t captures the individual difference in wage t years after the plant closure and the average wage in the two pre-displacement years.

Results for the first differences are presented in figure 2.4. The subfigures a and b of figure 2.4 show again the wage effect (the vertical axis is rescaled for better illustration), and the subfigures c and d show the regression results of $\overline{wage}_i$, which corresponds to the selection effect. Finally, the subfigures e and f show the regression results of $(wage_{it} - \overline{wage}_i)$. This can be interpreted as the net wage effect. The estimated DiD and the corresponding t-values are given in table 2.4.

First, I will discuss the results solely for workers displaced from large firms, subsequently I will discuss the results for workers displaced only from small firms. Results for workers displaced from large firms are presented in the subfigures in the left column of figure 2.4 and column one to three in table 2.4. The results indicate that workers displaced from large firms suffer a 1.8 p.p larger wage decline during the first post-displacement year if the plant closure occurs during a boom rather than during a recession (column 1 of table 2.3. In the second year workers displaced during a recession suffer a stronger wage decline than workers displaced during a boom, which leads to an insignificant DiD estimate for wages in the second year. In the last four years there are no significant differences in the wage effect between workers displaced during a boom versus those displaced during a recession. Subfigure c shows that over the whole observation period there are no statistically significant differences in the selection effect between the boom and the recession. This implies that the composition of PC workers who find a new job is independent of the business cycle for workers displaced from large firms. Therefore, the larger wage decline during the first post-displacement year for workers displaced during a boom is induced by a larger net wage decline (subfigure e). In the last four observed years there are hardly any differences between boom and recession in the wage effect, the selection effect and the net wage effect. Therefore, the according DiD estimates are statistically equal to zero. The wage losses for workers displaced from large firms are only affected by the business cycle in the short run.

In the pre-displacement years the wage differences between PC and NPC workers decline slightly over time. One possible explanation for this finding is that large firms try to avoid an imminent bankruptcy by reducing the wages of their employees. Comparing the composition of workers (subfigure c) of the last pre-displacement year with the first post-displacement year shows that, on average, a worker who finds a new job directly after displacement has had higher wages in the two pre-displacement years than the average worker in the year directly before the plant closure. In other words, high wage workers displaced from large firms have somewhat better chances to find a new job in the first post-displacement year than low wage workers displaced from large firms, independent of the business cycle.

Now the results for workers displaced from small firms will be discussed, shown in the subfigures in the right column of figure 2.4 and the last three columns in table 2.4. Workers displaced during a boom suffer a 5 p.p. larger wage decline during the first year than workers displaced during a recession (column 4 in table 2.4). Thereafter, the DiD estimates declines more or less continuously over time to 2 p.p five years after the displacement. In contrast to the findings for workers displaced from large firms, workers displaced from small firms suffer larger wage decline even in the long run if the plant closure occurs during a boom instead of during a recession.

The findings for the selection effect (subfigure *d*) show that if the plant closure occurs during a recession high wage workers find a new job first, whereas during a boom an average wage worker finds a new job first. This yields a positive estimated DID of about 2 p.p. for the selection effect from two to six years after the displacement, in contrast to the findings for workers displaced from large firms. Subfigure *f* shows that workers displaced from small firms during a boom suffer a net wage decline of almost 5 p.p. during the first year, but no additional net wage decline during the next five years. Workers displaced during a recession suffer a yearly wage decline of around 1.2 p.p. during the first four years. The milder net wage decline of workers displaced during a recession rather than during a boom leads to significant positive DiD estimates in the net wage effect during the first three years after displacement. The DiD estimates become insignificant during the last three years.

During the first three post-displacement years, there are two reasons why workers displaced from small firms during a boom suffer a higher wage loss than those displaced during a recession. First, high wage workers have a harder time finding a new job if the plant closure occurs during a boom rather than during a recession. Second, workers displaced during a boom suffer a higher net wage decline during the first three years after the plant closure. From the fourth to the sixth post-displacement year the positive DiD estimates in wages are only due to the fraction of high wage workers who find a new job first, which is higher during a recession than during a boom.

Again, results indicate that the labor market treats workers displaced from small firms differently, depending on whether they are displaced during a boom or during a recession. In the short run all workers displaced during a boom are punished by higher net wage declines than workers who are displaced during a recession. It seems to be the case thought that only high wage workers are punished in the long run. Even six years after displacement, high wage workers displaced during a boom have more problems finding a new job than high wage workers displaced during a recession. This result may indicate that high wage workers are held most responsible for the bad performance of a firm which in turn leads to the plant closure. Low wage workers are also punished by larger net wage declines if they are displaced during a boom rather than during a recession.

Krashinsky (2002) showed that the difference between the earnings losses of laid off workers compared with the earnings losses of displaced workers found by Gibbons and Katz (1991) becomes insignificant after one controls for the size of the former and the new employer. To take this into account, I have run two regressions with the wage as dependent variable (specification 2.2) where the size of the new employer is included as an additional regressor. The results are shown in table A.1. The DiD estimates in wages for workers displaced from large firms are now insignificant over the whole post-displacement period. The only difference to the results where the size of the new employer is ignored, is that the DiD coefficient for the first year after displacement is insignificant too. This is even stronger evidence for the hypothesis that workers displaced from large firms do not carry a negative signal at all, as suggested by Gibbons and Katz (1991). Conversely, the DiD estimates for workers displaced from small firms are even larger when one includes the size of the new employer. In other words, the results found in this chapter are not affected by the size of the new employer.

2.6 Conclusions

Most previous empirical studies on plant closure focused on earnings losses at one specific point in the business cycle. Nakamura (2004) showed for the first time within the framework of a theoretical model that earnings losses are larger when the plant closure occurs in a boom period instead of in a recession period. To my knowledge there is no paper analyzing if there are different effects between workers getting displaced from small firms compared to large firms. The displacement is a stronger signal about individuals' productivity for workers getting displaced from small firms than from a large firm. Furthermore, there are different signals of getting displaced at the beginning of a boom period than at the beginning of a recession period.

In this chapter, I show that in Austria workers displaced from small firms suffer significantly higher declines in earnings when they lose their job at the beginning of a longer boom period than at the beginning of a longer recession period. The larger earnings losses in boom are hardly driven by the according larger declines in wages. This finding goes hand in hand with the findings from Nakamura (2004) for the United States. For workers getting displaced from large firms the results are opposite. These displaced workers suffer larger earnings losses when they get displaced at the beginning of a recession period. The difference between the boom and the recession is strongly driven by large differences in the UP, in contrast to the findings for small firms where the UP effect does not depend on the business cycle.

Separating the wage effect in a selection effect and a net wage effect gives evidence that workers displaced from small firms in boom get punished. Moreover it seems that high wage workers get punished heavier than low wage workers. For displaced workers from large firms and from small firms in recession there seems to be no such punishment. This is evidence that these workers do not carry a bad signal from the displacement, as proposed by Gibbons and Katz (1991).

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2.A Appendix

Table A.1: DiD Wages, including the size of the new employer

Sample	Large firms	Small firms
Dep. Variable	Wages	Wages
Year -6	-0.003	-0.003
	(0.45)	(0.31)
Year -5	-0.002	0.004
	(0.28)	(0.46)
Year -4	-0.006	0.003
	(0.86)	(0.36)
Year -3	-0.007	0.016
	(1.10)	(1.85)
Year -2	-0.006	0.010
	(0.85)	(1.26)
Year -1	-0.003	0.013
	(0.40)	(1.61)
Reference Date	-0.009	0.005
	(0.72)	(0.32)
Year 1	0.007	0.058
	(0.96)	(5.65)**
Year 2	-0.004	0.052
	(0.56)	(5.40)**
Year 3	-0.008	0.041
	(1.10)	(4.27)**
Year 4	-0.006	0.030
	(0.77)	(3.08)**
Year 5	-0.008	0.034
	(1.06)	(3.43)**
Year 6	-0.008	0.019
	(1.09)	(1.83)
Obs.	3704037	956995
R^2	0.31	0.27

Notes: **, * denotes significance at the 1%, 5% level respectively. T-values in parentheses. UP is the unemployment probability times 100.
Source: Own calculations based on ASSD.

CHAPTER 3

Wages and Risks at the Workplace: Evidence from Linked Firm-Worker Data

joint with Rafael Lalive and Josef Zweimüller

> "Virtually no matched worker-firm records are available for empirical research, but obviously are crucial for the precise measurement of job and personal attributes required for empirical calculations. Not only will the availability of such data produce sharper estimates of the wage-job attributes equalizing differences function, but also will allow more detailed investigations of the sorting and assignment aspects of the theory."
>
> Rosen (1986), p.688.

3.1 Introduction

One of the oldest views on wage determination holds that observed differences in wages reflect compensation for non-wage aspects of jobs. One aspect that has received a considerable amount of attention in the previous literature is the compensation of the risk of workplace injuries. This chapter reconsiders the importance of such compensating wage differentials using linked firm-worker data reporting both workers' injuries and workers' earnings. Twenty years ago almost no linked firm-worker data were available. In recent years new data sets of that kind have been increasingly used to study labor market phenomena (for a survey, see Abowd and Kramarz, 1999). While these data have been mainly used to study the (firm- and worker-specific) determinants of wages, previous research has not yet looked at (firm- and worker-) determinants of non-wage aspects of jobs and the repercussions on wages. In this chapter we shed new light on the empirical evidence concerning the joint (firm- and worker-) determinants of wages and workplace risks.

We argue that linked firm-worker data allow us to identify the building blocks of the theory – the wages and workplace risks attached to a firm. The risk of injury results from the interplay between the risk inherent in the workplace and the risk and the mobility behavior of the worker. The theory of compensating wage differentials holds that workers exposed to higher injury risks should be compensated by higher wages. In contrast, there is no reason to compensate a worker for risk that is attached to his or her behavior (reflecting lack of precaution, dexterity, or diligence) unless this increases his or her productivity.

Our empirical analysis relies on a unique administrative data set from Austria. This data set covers the universe of Austrian male blue collar workers and their firms (about 620,000 individuals employed in more than 60,000 firms). A unique feature of our data is that we can observe, for each individual, the number, the exact timing (on a daily basis) and the severeness (as measured by lost work days) of workplace accidents over the period January 2000 to December 2002. Since our data set covers the universe of Austrian employees and since we know the workers' complete earnings and employment history across different firms, we have also exhaustive information on injuries at the firm-level. Exploiting this information allows us to disentangle risks and wages attached to the *firm* from risks and wages specific to the *worker*.

Our empirical strategy is as follows. Using econometric techniques developed by Abowd *et al.* (2002) we split up observed wages into a (time-constant) worker-effect and firm-effect. We then proceed in an analogous way for observed injuries to disentangle the risk inherent to the workplace from the risk attached to the behavior of the worker. While injury incidences are not very informative at the individual level – workplace injuries are rare events and mainly reflect good or bad luck – we argue that these measures are informative at a higher level of aggregation. A similar problem arises for small firms. Hence, while the frequency of injuries may largely reflect the true injury risk of a workplace, the small frequency of injuries in smaller firms also call for aggregation to a higher level. With aggregation, the random component is averaged out and variation in injuries across labor market cells yield information on variation in the risk exposure of workers.

Our empirical strategy adds two new elements to existing studies. First, by identifying the relationship between wages and injury risks that are specific to the firm it identifies the building blocks of the theory and provides refined empirical estimates for the hedonic wage function. Second, by wages and risks that are attached to the worker, we can also address the issue of how workers sort themselves across different workplaces. In particular, we can study whether workers with a high earnings capacity choose more secure jobs and whether workers willing to take high risks (or high ability to avoid risks) sort themselves into high-paying jobs.

Our empirical analysis yields the following results. *First*, our results indicate that, consistent with the theory of compensating wage differentials, firm wages and firm risks are positively correlated. This result is robust and holds for different subsamples and alternative measures of workplace risks (incidence versus duration; and aggregation level of the risks measure). Quantitatively, our preferred estimates imply a value of a statistical (non-fatal) injury equal to $20,000, which is on the lower end of the $20,000 to $70,000 found for other industrialized countries (Viscusi and Aldy, 2003). *Second*, our empirical strategy also allows us to shed new light on the sorting of workers across firms that offer different workplace (dis)amenities. We find no evidence in favor of a sorting of high-productivity workers into low-risk jobs. This suggests that the bias of the compensating differential obtained from a standard cross-sectional hedonic wage function that is due to unobserved productivity of workers is small. However, we find that, conditional on firm-risk, high-risk workers sort themselves into high-wage jobs. This is consistent with an explanation that workers who are willing to take high risks also accept other workplace disamenities (and be compensated for them).

The chapter is organized as follows. Section 3.2 briefly discusses related literature. Section 3.3 presents a simple model of wage determination where workers differ both in productivity and in the ability to avoid workplace accidents; and firms differ in the wages and the riskiness of workplaces they offer. Section 3.4 presents the data and gives first descriptive evidence on the relationship between the injury risk and earnings. Section 3.5 gives some institutional background on wage determination and insurance against workplace accidents in Austria. Section 3.6 briefly discusses the econometric methodology to disentangle worker- and firm-effects for both wages and injury risks. Section 3.7 presents the main results and checks their robustness. Section 3.8 concludes.

3.2 Related Literature

Among the early papers studying the impact of job-injury risk on earnings on the U.S. labor market are Thaler and Rosen (1975), Brown (1980), Leigh (1981), and Arnould and Nichols (1983) who use risk data collected by Society of Actuaries for 1967 whereas Hamermesh (1978), Viscusi (1980), and Fairris (1989) find that self-reported riskiness of one's job is significantly positive related to an individual's wage. Duncan and Holmlund (1983), using Swedish data, show that longitudinal data are necessary to reveal any significant compensating differential of various job disamenities.

A major issue in the empirical literature of estimating compensating wage differentials is the issue of sorting. Many authors have argued (e.g. Brown, 1980; Hwang *et al.*, 1992) that worker's productivity characteristics (such as talent and innate ability) that are unobservable to the researcher may bias the estimated compensating differential downward. When workplace safety is a normal good, workers with high earnings potential will select themselves into less risky jobs. Using simulation techniques Hwang *et al.* (1992) show that, when such differences in unobservable productivity are not accounted for, the estimated compensating differential may be strongly downward biased and even lead to "wrongly-signed" coefficients. However, Shogren and Stamland (2002) show that when workers differ in the ability to avoid risk, the standard hedonic wage regression may bias the compensating wage differential upwards. They show that also this bias could be very large. Garen (1988) emphasizes that individuals may systematically differ in productivity-relevant characteristics that are specific to dangerous jobs. "Coolheadedness" makes workers more productive on a dangerous job but "coolheadedness" may not be relevant in a safe job. Garen (1988) and DeLeire and Levy (2004) use family characteristics as instruments elaborating the idea that being responsible for others lets individuals choose less dangerous jobs. These papers find evidence for systematic sorting of workers into jobs.[1]

Linked firm-worker data have not been used to study the importance of compensating wage differential for workplace injury risks. The only exception known to us is Dale-Olsen (2006) using linked firm-worker data from Norway. The focus in his paper is on dynamic aspects of worker- and firm-behavior such as quits and job durations whereas our paper focuses on the issue of disentangling worker- and firm-effects in the hedonic wage equation.

3.3 A Simple Model

Consider a simple model of wage determination and non-fatal injury risk, inspired by Viscusi and Aldy (2003). Denote by w a worker's wage and, respectively, the utility with and without an injury by $U(w)$ and $V(w)$ with standard assumptions $U'(w) > 0 > U''(w)$ and $V'(w) > 0 > V''(w)$. The probability that an injury occurs is given by p. Assume for simplicity that the utility in the case of an injury $V(w) = (1-k)U(w)$ where k should be thought of as a measure of the severeness of an injury. This lets us write the worker's expected utility as $EU = [1 - pk]\, U(w)$.

Now assume that the hedonic wage function is linear and given by $w = h + \beta p$ where h denotes a worker's earnings capacity and β is the compensating wage differential. Substituting the budget constraint into the expected utility expression, the optimal level of risk chosen by the worker is implicitly given by the first order condition

[1] A further related strand of the literature explores to which extent observed industry wage premiums are associated with wages compensating for on-the-job risk. Leigh (1995) and Dorman and Hagstrom (1988) compare models with and without dummy variables for industry affiliation and conclude that industry-wage differentials reflect to a large extent risk-premiums. The recent literature addresses the problems with measuring compensation for risk. Ashenfelter and Greenstone (2004) and Ashenfelter (2006) use mandated speed limits to measure the value of a statistical life. Heliwell and Huang (2005) use information on life satisfaction, wages, and workplace characteristics to identify compensating wage differentials.

$$\frac{\partial EU}{\partial p} = -kU(w) + [1 - pk]\,\beta U'(w) = 0.$$

Using this condition, we can now easily study how the optimal level of risk-taking varies with a worker's earnings capacity. Implicitly differentiating the p with respect to h shows that

$$\frac{dp}{dh} = -\frac{1}{\beta}\frac{kU'(w) - [1 - pk]\,\beta U''(w)}{2kU'(w) - [1 - pk]\,\beta U''(w)} < 0.$$

A workers higher earnings capacity unambiguously reduces the degree of risk that workers are willing to take. A corollary of this result is that, when variation in wages is to a large extent due to unobserved productivity of workers, estimating compensating wage differentials from cross-sectional data suffer from ability bias and will underestimate the true compensating differential.

A further potentially important dimension of sorting concerns the ability of individuals to cope with risky workplaces. The reason why an industry can have a high number of injuries is twofold. On the one hand there are differences in injury rates because there are differences in risks embodied in the workplace. This is emphasized by the standard theory of compensating differentials. On the other hand, there are differences in industry injury rates because different industries select different workers. When workers differ in their ability to cope with workplace risks – because of differences in worker's dexterity, precaution, and diligence – there may be further sorting of workers across high- and low-risk industries. It is straightforward to explore this argument in the context of the above model. Assume workers differ in the ability to avoid an injury and that the probability that a worker of type π experiences an injury is given by

$$p(\pi) = p\pi$$

where $\pi > 0$ says that a worker of type π is π times as likely to experience an injury than the average worker. Of course, it must be that $\pi^{\max} < 1/p$.

The worker now maximizes $EU = [1 - p\pi k]\,U(w)$ taking the hedonic wage equation $w = h + \beta p$ as given.[2] The first order condition is now

$$\frac{\partial EU}{\partial p} = -k\pi U(w) + [1 - p\pi k]\,\beta U'(w) = 0.$$

[2] One could further assume that it matters for the productivity of a worker how able he or she is in coping with workplace risks. Realistically, the occurrence of an injury will be associated with lower output, so the expected productivity of a high-risk worker will be lower. On a perfect labor market (where a worker's risk is common knowledge) this should result in a lower output. Hence, ceteris paribus, a worker with low ability to cope with workplace risks should also get lower wages. To include this argument into the above framework we could assume, for instance, a hedonic wage equation of the form. $w = h + \beta p - \gamma\pi$. Using such a specification of the hedonic wage equation would lead to the same sorting patterns as discussed in the main text.

Implicitly differentiating the first order condition for p with respect to π yields

$$\frac{dp}{d\pi} = -\frac{kU(w) + pk\beta U'(w)}{2k\beta\pi U'(w) - [1 - p\pi k]\,\beta^2 U''(w)} < 0,$$

which implies that high-risk workers will sort themselves into low-risk workplaces.

3.4 Data

We assess the extent to which wages compensate for injury risks with linked employer-employee data from Austria. We use data from two different sources: (i) the Austrian social security data (ASSD[3]) and (ii) the Austrian statutory accident insurance (Allgemeine Unfallversicherungsanstalt, AUVA). These data sets were merged for the purpose of this study on an individual (and anonymized) basis. The available data include the universe of Austrian private sector workers who were employed at some date between January 1, 2000 and December 31, 2002. The ASSD reports the workers' complete employment and work history since January 1, 1972 and the AUVA-data report the complete history of occupational injuries (incidence and duration) between the period January 1, 2000 and December 31, 2002. Both data sets are linked via an individual identifier (the workers' anonymous social security number). The ASSD also contains a firm identifier from we can infer, on a daily basis, at which firms a worker is employed. Because our data set includes the universe of all private sector workers, we have exhaustive information not only on the workers' but also on the firms' history of occupational injuries (and the corresponding earnings) over the period January 1, 2000 to December 31, 2002.

Both the ASSD and the AUVA-data are administrative data set that is unlikely to suffer from measurement error. The ASSD contains all data necessary to calculate old age social security benefits. (Benefits levels depend both on previous earnings and on the number of months during which social security contributions were paid.) Since contributing to the old age insurance fund is mandatory and since non-compliance with reporting rules are subject to sanctions (fines), this data set contains high-quality information on workers employment and earnings history.

Our empirical analysis is confined to workers in the age group 25-65. As the data report daily earnings but do not provide information on average working hours per day, we focus on male workers (where variation in wages results predominantly from variation in hourly wages rather than variation in daily hours worked) and exclude female workers (many of whom work part-time). Moreover, we focus on blue collar workers because the social security contribution ceiling is not binding for this group – implying that top-coding of earnings is not a problem – so that we can use standard methods to decompose wages. Furthermore, occupational injuries are much more prevalent in blue collar jobs, we exclude

[3]For a detailed description of the ASSD see Kuhn and Ruf (2006)

white collars. We also excluded multiple job-holders, workers with wages below the social security threshold (*Mindestgrenze*) and above the social security earnings cap (*Höchstbemessungsgrundlage*).[4] Furthermore, as identification of firm- and worker-components in earnings and risks can only be accomplished for workers moving between firms, we could only concentrate on those subset of workers and firms for which identification of both worker and firm effects was possible.[5] We ended up with 618,125 male blue collar workers working in 62,497 firms. We split the three-years period 2000 - 2002 into six semesters (using wage observations at May 10 and November 10 of each year) and calculating flow variables (such as the number of injuries, days of work experience, etc.) on a semesterly basis. In total this leads to 2,845,770 observations.

Table 3.1 reports descriptive statistics for this sample. The yearly risk of an injury per 100 full-time workers is 7.2 percent. Notice that this is a rather high number when compared to statistics from other data sources. The reason is twofold. First, we focus on male blue collar workers, a group that is typically employed in more risky workplaces. Second, we define the risk of an injury as the number of workplace injuries within one year, divided by the number of calendar days during which workers were *in employment*. This implies that spells of unemployment (or non-employment) are not counted in the denominator of the risk-variable. The number of lost working days per 100 workers is about 97 days. Again, this number is somewhat higher than those found in other data sets because we focus on male blue collars. The average daily wage in the sample is about 70 Euros (or $100). On average, workers in our data set have 16.8 years of work experience since January 1972, were employed for 7.4 years by their current firm and are 39 years old. The size of the firm in which the typical worker is employed is 467.

3.5 Institutional Background

Our simple model above and our empirical analysis below implicitly assumes that wages and working conditions are the outcome of individual choices by workers and firms which interact on a perfect labor market. This is clearly a simplification in the Austrian context where wage determination is the outcome of negotiations between national unions and employer federations. Bargaining takes place at the industry level but there is coordination both among employer federations and among the various industrial unions. Each fall, yearly negotiations about wage raises take place between unions and employers in the metal industry. The outcome of which usually serves as a benchmark for the negotia-

[4]For earnings below the Mindestgrenze and the part of the earnings above the Höchstbemessungsgrundlage, workers do not have to pay social security contributions. For the former group records are incomplete. For the latter group we do not know the exact amount of earnings (we only know that earnings are above the cap). Applying both criteria lead to exclusion of 4014 workers.

[5]This requires that firms and workers are "connected" (see Abowd and Kramarz (1999)). Two subsets of the labor market are connected, if at least one worker moves from one subset to the other.

Table 3.1: Descriptive statistics

	Mean	Std. dev.
Injury risk	7.238	(44.703)
Duration	97.002	(983.642)
Wage rate	69.906	(20.112)
Work experience	16.753	(8.039)
Tenure	7.354	(7.396)
Age	39.073	(8.963)
Firmsize	466.723	(1583.463)
N(obs)	2,845,770	
N(firms)	62,479	
N(workers)	618,125	

Notes: Mean refers to the average over all 2,845,770 semester-observations. Injury risk measures the yearly number of injuries per 100 continuously employed workers. Injury duration measures the yearly number of lost workdays for 100 workers. Experience (tenure) measures the number of years spent in employment (with the current employer) since January 1972 (the starting point of our data).
Source: Own calculations based on ASSD and AUVA.

tions in other industries. As in many European countries, bargaining coverage in Austria exceeds union density. Negotiated wage increases are extended to all employees within the industry.[6]

Despite this seemingly strong influence of national trade unions on wage determination, wage dispersion in Austria is substantial. First, bargaining sets a wage floor ("Kollektivvertragslöhne") whereas actually paid wages typically deviate from this floor and only a small fraction of employees are paid the minimum wage. Second, the bargaining system is two-tiered: wages negotiated at the industry level may be readjusted by subsequent negotiations at the firm-level leaving room for firm-specific circumstances which will partly include job amenities. Finally, while negotiations also include wage raises for job stayers paid above the minimum wage ("Istlöhne"), much wage dispersion arises from individual raises (above the negotiated raises) and/or from job movers.[7] As

[6]Austria is often considered as having one of the most centralized systems of wage determination (Calmfors and Driffill (1988) and OECD (2004)), a view that is not shared by insiders (see e.g. Pollan (2000)).

[7]The centrally negotiated industry wage sets a lower limit, the actual wage outcome may well be higher

a result, bargaining outcomes are rarely a binding constraint for wages individually negotiated between the firm and the worker. In sum, while the bargaining system looks prima facie strongly regulated, there is substantial wage dispersion potentially reflecting compensation for differences in workplace amenities.[8]

There is mandatory insurance against workplace accidents and occupational diseases by which all dependent employees (and, in case of a fatal injury, their family dependants) are covered. Workers are eligible to disability benefits when a workplace accidents leads to inability of work for more than 3 months. The level of these benefits depends on the extent of the disability (as measured by the degree of incapacity of work) between 13 percent of the previous wage (for 20 percent work incapacity) and 100 percent of the previous wage (for 100 percent incapacity). Moreover, the worker is eligible to full coverage of all expenses related to medical treatment and/or rehabilitation of workplace accidents.[9]

3.6 Methodology and Empirical Strategy

This section discusses the decomposition of the log of earnings per day into a worker effect, a firm effect, and an error term. Moreover, the section discusses how to separate the risk of injury into worker and firm effects.

Decomposing Wages Let w_{it} be the log earnings per day of worker i at time t, let x_{it} denote the time-varying characteristics, and let $J(i,t)$ be the identification number of the firm at which worker i is employed at time t. We assume that

$$w_{it} = x_{it}\beta^w + \theta_i^w + \psi_{J(i,t)}^w + \epsilon_{it}^w \tag{3.1}$$

and

$$E[\epsilon_{it}^w | i, t, J(i,t), x_{it}] = 0. \tag{3.2}$$

The wage policy of the firm is modeled as simple as possible.[10] The firm effect in the wage

as bargaining rules leave scope for subsequent firm-level negotiations. According to personal information from the union officials of the "Gewerkschaft Metall Textil Nahrung" of the Austrian Labor Union (OeGB), only 4.7 percent of all workers in the metal industry are paid the 'minimum wage' and subsequent firm-level negotiations lead to agreements which have been up to 1.5 percent above the industry wage settlement depending on firm performance.

[8] See Duncan and Stafford (1980) for a theoretical analysis of compensating differentials when wages are determined under collective bargaining.

[9] The fact that workers are compensated in terms of disability benefits implies that the compensated wage differential estimated on wage data only underestimates the true market value of risk. See Arnould and Nichols (1983) for an analysis of how including workers' insurance against workplace accidents affects the compensating wage differential.

[10] A more elaborate model for the wage policy allows for firm-specific returns to tenure. However, keeping the wage policy of the firm as simple as in equation 3.1 allows identifying the wage policy for a larger number of firms because only one parameter per firm needs to be estimated. Moreover, allowing for firm-specific returns to seniority does not affect results of the wage decomposition (Gruetter and Lalive,

rate, ψ_j^w, captures the wage differential earned in the present firm compared to the average firm in the data set. The worker's wage component, θ_i^w, reflects differences in pay due to time-invariant characteristics of each worker such as ability but also education.[11] Thus, the worker effect in the wage rate measures the extent to which compensation for skill is important. Finally, the parameter β^w measures economy-wide returns to experience or productivity increases (see the following section for a definition of the vector x).

Intuitively, the worker's effect on the wage and the firm's effect on the wage can be separated by observing workers moving between firms. The wage change associated with a job change provides information on the firm effect of the new firm relative to the firm effect in the old firm. The main statistical assumption is the assumption of exogenous mobility between employers (equation 3.2). This assumption basically ensures that the model is identified. The exogenous mobility assumption rules out correlation between unmeasured time-varying effects on the wage rate captured by ϵ_{it}^w with the person effect θ_i^w, the firm effect $\psi_{J(i,t)}^w$ or the time varying observed effects x_{it}. Note, however, that this assumption does not rule out that workers move to better paying firms. Correlation between the firm effect and the mobility decision does not imply that the assumption of exogenous mobility is invalid. Furthermore, in previous work we find that endogenous mobility does not lead to a strong bias in decomposing wages (Gruetter and Lalive, 2004).

Direct estimation of the model (3.1) by least squares is impossible because this is a large two-way fixed effect problem. While we can eliminate the worker fixed effect by taking deviations from worker means, there are still more than 60,000 firm effects that need to be estimated (Abowd and Kramarz, 1999). This chapter uses a modified version of the iterative algorithm proposed in (Abowd *et al.*, 2002) to solve for the least squares parameter estimates $\widehat{\beta}, \widehat{\theta}_i$, and $\widehat{\psi}_j$ (see appendix for a description of our algorithm).

Decomposing Injury Risk This chapter argues that workplace injuries are generated by factors which are firm specific (technology, safety regulations, work stress, ...) and by factors, which are worker-specific (precaution, ability, skill level, ...). Because workers need to be compensated only for the risk of injury to which they are exposed on the job, we are also interested in measuring the firm's contribution to the risk of an injury or illness. We therefore propose the following statistical model for the relative risk of an injury or illness of worker i in the half-year t, R_{it}

$$R_{it} = x_{it}\beta^R + \theta_i^R + \psi_{J(i,t)}^R + \epsilon_{it}^R \tag{3.3}$$

and

$$E[\epsilon_{it}^R | i, t, J(i,t), x_{it}] = 0. \tag{3.4}$$

2004).

[11] Recall that our data do not have information on education.

Equation (3.3) implies that the risk of an injury or illness is generated by time-varying individual characteristics, x_{it}, by the firm-effect in risk ψ_j^R, and by the worker effect in risk θ_i^R. The exogenous mobility assumption (3.4) is required for identification of this model. We estimate this model again using the iterative algorithm that finds the least squares solution. Fitting ordinary least squares is appropriate even though the dependent variable is censored at zero because we are interested in measuring the expected relative risk conditional on the characteristics x, the firm identifier, and the worker identifier. (We do not have a model for underlying propensity to have an injury.) Moreover, note that the average predicted risk of a worker or a firm turns out to be non-negative because ordinary least squares fits the average risk of each worker or firm. Thus, there is no problem with 'non-sensical' risk predictions.

Constructing firm- and worker-risk indicators Note that our estimates of the underlying worker risk and, for small firms, the underlying firm risk is noisy because our data only covers a three year period. For instance, a worker who happens to have an accident in half-year t will have a very high estimated worker effect $\hat{\theta}_i^R$ even though the underlying true worker effect might be small. Similarly for small firms. This is a problem, however, that is common to all objective measures of risk. The literature has commonly dealt with this problem of noise by aggregating the risk measure either to the level of the industry or occupation (Viscusi and Aldy, 2003).

Our data set gives us some choice about the level of aggregation to which our risk indicators are aggregated. In our main results, we proceed as follows. For larger firms (with 50 or more employment observations) we take the estimated firm effects, $\hat{\psi}_{it}^R$, as the relevant risk indicator. For smaller firms (less than 50 employment observations) we aggregate the estimated effects ($\hat{\psi}_{it}^R$ and $\hat{\theta}_i^R$) within labor market cells (based on industry, firm size and region). This lets us end up with high variation in exposure to risk and at the same time avoids measurement problems due to the randomness of individual injury events. The average firm risk within a labor market cell captures the average risk to which workers are exposed, given the characteristics of their workplace. Arguably, this is the component of overall risk in the industry that needs to be compensated. In contrast, the worker risk within a labor market cell captures the average willingness to take risks (or the average ability to avoid risks) of workers in a particular labor market segment. Importantly, these aggregated firm and worker risk measures are much less polluted by noise than the individual data. We can therefore reliably assess the compensating wage differential for risk using the industry risk measure.

Table 3.2 shows descriptive statistics for the frequency of injuries on the individual level, on the firm level, and on the level of aggregation to labor market cells for small firms. Column 1 shows the distribution of injuries across the 618,125 workers in our sample. The majority of these workers, 87.5 percent, do not experience an injury during the observation period; 10.7 percent experience one injury, and 1.8 percent of the workers experience two or more injuries during the observation period. Column 2 shows the distribution of injuries across the 62,479 firms in our sample. 72.6 percent of all firms do not "produce" an injury during the observation period, 11.7 percent produce one injury, 4.7 percent produce two injuries and 11 percent of all firms produce three or more injuries. The large fraction of firms with no injuries is driven by the large number of very small firms in our sample. To avoid the noisiness of risk measures, our aggregation of firms with less than 50 observations into an industry-size-region labor market cell reduces the number of risk units to 11,134 labor market cells. In only 20 percent of these cells there was no injury during the observation period, 25 percent have one or two injuries, another 26 percent have 3 to 6 injuries, and about 29 percent of all cells have more than 6 injuries. In what follows we will take the labor market cells of column 3 as the relevant unit within which the risk exposure is measured. However, we will come back to the aggregation issue in the sensitivity analysis where we will show how our results change when we use alternative (finer or coarser) levels at which risk is measured.

Table 3.2: Injury incidence, by workers, firms and labor market segments

	Workers		Firms		Labor market segments	
	#	%	#	%	#	%
0	540,600	87.46%	45,334	72.56%	2251	20.22%
1	66,266	10.72%	7292	11.67%	1600	14.37%
2	9,397	1.52%	2940	4.71%	1216	10.92%
3	1,514	0.24%	1548	2.48%	955	8.58%
4	268	0.04%	1082	1.73%	814	7.31%
5-6	72	0.01%	1281	2.05%	1126	10.11%
7-9	8	0.00%	981	1.57%	949	8.52%
10-14			774	1.24%	791	7.10%
15-24			612	0.98%	649	5.83%
25-49			437	0.70%	499	4.48%
50+			198	0.32%	284	2.55%
Total	618,125	100.00%	62,479	100.00%	11,134	100.00%

Source: Own calculations based on ASSD.

3.7 Estimating the Compensating Differential for Injury Risk

In this section we use the method described above to decompose both observed incidence of workplace accidents and observed wages into firm- and worker-effects and use these estimated effects to explore the relationship between workplace risks and wages. Taking the estimated worker fixed effects and the estimated firm fixed effects at face value we can shed new light on the empirics of compensating wage differentials. In particular, we interpret the fixed firm effect in the wage equation – to which we will refer as the "firm wage" – as reflecting compensation for workplace (dis)amenities associated with the risk of a workplace injury; and we interpret the fixed firm effect in the risk equation – the "firm risk" – as the risk that is attached to the workplace. Under the assumption that firm wages are true compensations for workplace disamenities (including injury risks) and firm risks measure the true exposure to injury risk on the workplace, the correlation between the two components identifies a compensating wage differential for the risk of workplace injuries.

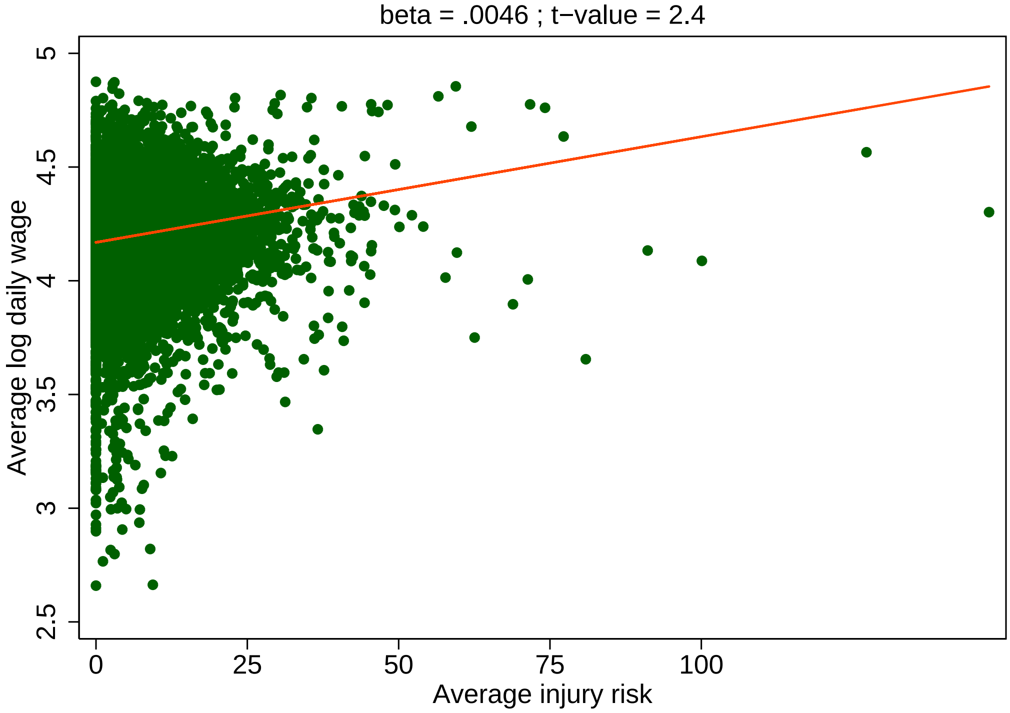
Similarly, we interpret the estimated worker fixed effect in the wage equation – the "worker wage" – as a measure of a worker's productivity and the estimated worker fixed effect in the risk equation (at some level of aggregation) – the "worker risk" – as a measure of the willingness to take a risky job (or the ability to avoid an injury) of the average worker. Under the assumption that estimated firm- and worker-effects represent true productivities of and true risks attached to jobs and individual workers, we are not only able to estimate the compensating differential, but also to characterize the sorting of workers across jobs with different workplace risks.

3.7.1 Main Results

The standard hedonic wage regression A meaningful starting point is the standard hedonic wage regression. Figure 3.1 plots the mean log-wage against the annualized mean incidence of workplace injuries within each of our 11,134 labor market cells. (Recall that, for larger firms, a cell is the firm itself and, for smaller firms, a cell is an industry-region aggregate.) We aggregate the observed wage and the observed injury incidence into labor market cells and plot the resulting means against each other. The number of data points in figure 3.1 is equal to the 11,134 data labor market cells in our data. The figure shows a positive and significant correlation between the two variables, albeit there is considerable variation both in wages and in injuries between cells.

Table 3.3 presents the results of a standard hedonic wage regression that controls for individual characteristics (age, tenure, and calendar time). Column 1 is based on a simple cross-sectional regression, whereas column 2 presents results from a regression that controls for individual worker fixed effects. As indicated in these regressions, the

Figure 3.1: Wages and injury risks, industry x firmsize x region categories



Notes: Vertical axis measures log daily wage, horizontal axis measures injury risk. Both measures are aggregated on (plant size)x(region)x(industry) cells for firms with less than 50 observations within the observation period. For larger firms both measures are aggregated on the firm level. There are 11,134 different risk relevant categories.
Source: Own calculations based on ASSD and AUVA.

workplace injury risk of a worker's labor market cell has a positive and significant effect on the wage. Unlike previous studies (e.g. Duncan and Holmlund, 1983)) we find that the point estimate of fixed effects regression is smaller than the point estimate in the OLS regression. (If workers with a higher fixed wage effect choose safer workplaces, the OLS coefficient has an upward bias; in that case the fixed effect estimate should be larger than the OLS-coefficient.) However, we will see below that this result stems from only allowing for fixed worker-effects but neglecting fixed firm-effects.[12] Quantitatively, the estimated OLS coefficient of column 1 is equal to 0.0056, which implies that, for avoiding one additional accident in 100 within a year, a worker is willing to sacrifice roughly 0.56 percent of his yearly earnings. Put differently, to avoid 1 injury per year, 100 workers would be willing to pay 0.6 times a yearly income. A yearly income of an Austrian blue collar worker is about 25,000 Euros ($35,000). The implied value of a statistical injury is then roughly 14,000 Euros ($20,000). These numbers compare to the range of $20,000 to

[12]Our data set does not contain information on the worker's formal education. However, the vast majority of blue collar workers leaves the education system after 9 years of schooling (i.e. after the end of mandatory schooling) and enters some kind of vocational training ("Lehrausbildung") thereafter.

Table 3.3: Effect of injury risk (disaggregated) on wage

Dependent variable	ln(wage)	
	OLS	FE
Injury risk	0.0056***	0.0029***
	(10.58)	(90.43)
Age	0.0025***	0.0183***
	(3.83)	(41.57)
Age squared / 100	−0.0043***	−0.0234***
	(−5.17)	(−52.27)
Tenure	0.0285***	0.0073***
	(27.48)	(85.95)
Tenure squared / 100	−0.0596***	−0.0197***
	(−16.10)	(−45.74)
Constant	3.9614***	3.7832***
	(277.23)	(318.07)
Time dummies	YES	YES
Observations	2,845,770	2,845,770
R^2	0.136	0.057

Notes: ***, **, * denotes significance at the 0.1%, 1%, 5% level respectively. Robust t-values in parentheses. Injury risk is aggregated on (plant size)x(region)x(industry) cells for firms with less than 50 observations within the observation period. For larger firms the injury risk is aggregated on the firm level. There are 11,134 different risk relevant categories.
Source: Own calculations based on ASSD and AUVA.

$70,000 reported in Viscusi and Aldy (2003) for other industrialized countries.[13] Hence our Austrian estimates are within the range of estimates obtained in previous studies. One possible reason why our estimate is on the lower end of this range is a rather generous accident and health insurance system provided by the Austrian welfare state.

Firm-wage, worker-wage, and workplace risks In figure 3.2 we extend the standard procedure in previous studies by looking separately at the effect of observed injury risks on the firm-wage and the worker-wage. More precisely we first decompose, for each observation in our data set, the observed wage into a firm- and a worker-effect using the techniques described in the last section. We then aggregate these estimated effects into

[13] Most of the regression reviewed in Viscusi and Aldy (2003) are based on cross-sectional hedonic wage regressions. Hence the main text compares the value of a statistical injury implied by the OLS-coefficient. The corresponding value on the basis of risk-coefficient estimated by the fixed-effects model would only be half as large.

labor market cells and plot the means of the estimated firm- and worker-wages against the observed incidence of workplace injuries in the respective cell (just like figure 3.1, figure 3.2 displays 11,134 data points). Panel A of figure 3.2 shows the relationship between the (cell mean of the) estimated *firm* wage-effect and the (cell mean of the) observed (annualized) injury-risk; Panel B of figure 3.2 presents the corresponding relationship between the (cell mean of the) estimated *worker* wage-effect and the (cell mean of) the observed injury-risk. From panel A, we see that there is a positive relationship between the *firm*-wage component and the injury risk. This is consistent with the theory of compensating wage differentials. From panel B we do not see such a relationship between the *worker*-wage component and the cell workplace-injury risk.

Table 3.4 shows corresponding regression results when we control for individual characteristics. Just like the regressions in table 3.3, columns $1-3$ of table 3.4 control for age, tenure, and calendar-time. For ease of comparison, column 1 repeats the risk-coefficient of the standard (OLS) hedonic wage function of table 3.3. In columns 2 and 3 we use the estimated firm- and a worker-effects as explanatory variables.

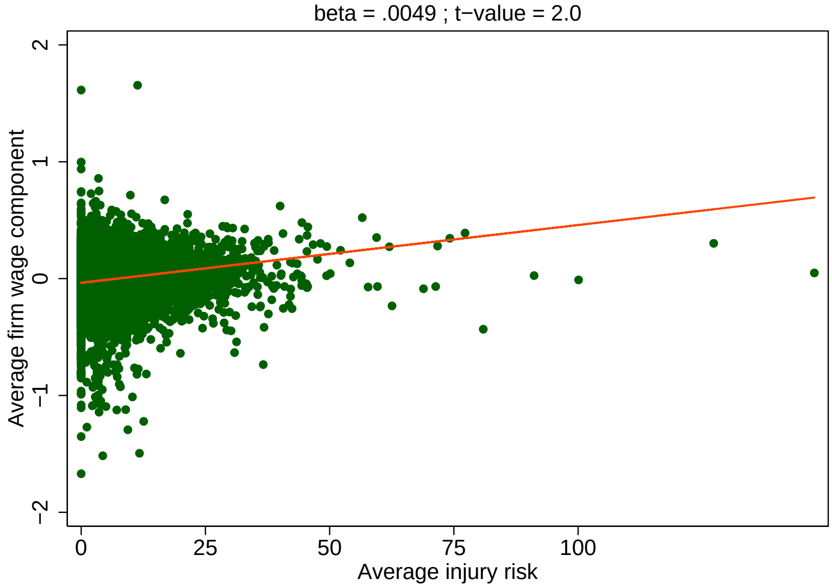
The firm wage regression in column 2 shows that the firm-wage is significantly affected by the observed injury-risk of the labor market cell. The risk-coefficient amounts to 0.005 and is almost identical to the OLS-coefficient of column 1. The fact that workplace risks and firm wages are positively correlated is clearly consistent with the theory of compensating wage differentials. From a theoretical point of view, any wage differential that compensates for workplace hazards should affect the firm component (but not the worker component) of the wage. To attract a worker of a given quality, the firm has to pay the compensating differential to ensure that the (marginal) worker is at least as well off at the current workplace as on any relevant alternative job.

Column 3 of table 3.4 regresses the injury risk indicator on the worker-wage. We do not expect a causal effect of the former on the latter but these two indicators should be correlated if there is sorting. For instance, one important hypothesis holds that, when workplace safety is a normal good, workers with higher earnings capacity are willing to sacrifice income in exchange of less risky jobs. Hence individuals with a high worker-wage should be found in low-risk jobs. The results in table 3.4 do not indicate support for this hypothesis. To the contrary, the point estimate of the risk-indicator does not show the expected negative sign. The point estimate is positive and significant (on the 10% level), though quantitatively small.

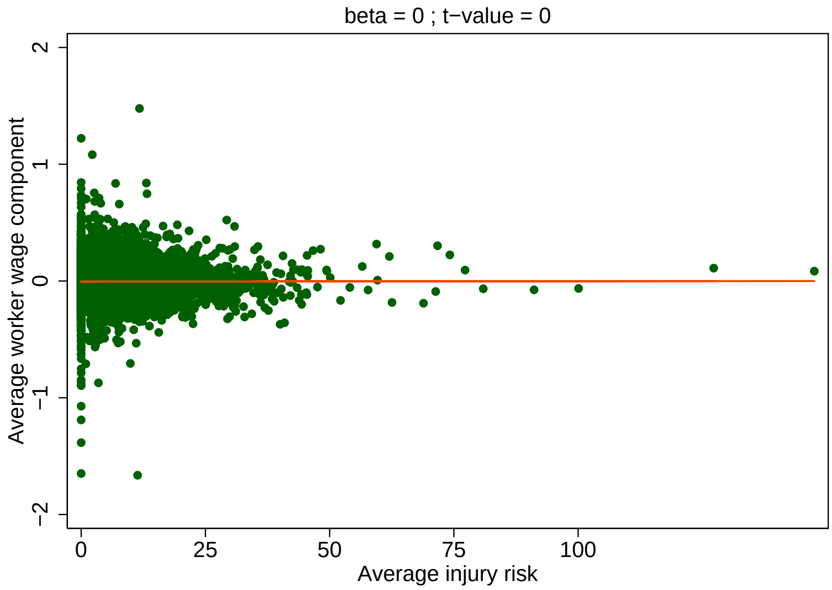
Columns 4-6 report the results from a set of regressions similar to those of columns 1-3. However, we now control also for industry and region. Because our risk indicator is constructed on the basis of industry/region labor market cells, allowing for variation in wages along these dimensions (on top of variation in workplace injuries) is potentially important. Hence these regressions provide a first check for the robustness of our results. It turns out that controlling for industry and region does not change the picture

Figure 3.2: Wage component and injury risk, industry x firmsize x region categories

A. Firm wage component and injury risk



B. Worker wage component and injury risk



Notes: Vertical axis measures wage components, horizontal axis measures injury risk. Both measures are aggregated on (plant size)x(region)x(industry) cells for firms with less than 50 observations within the observation period. For larger firms both measures are aggregated on the firm level. There are 11,134 different risk relevant categories.
Source: Own calculations based on ASSD and AUVA.

Table 3.4: Effect of injury risk (disaggregated) on wage component, additional controls

Dependent variable	ln(wage)	firm wage component	worker wage component	ln(wage)	firm wage component	worker wage component
Injury risk	0.0056***	0.0052***	0.0004	0.0039***	0.0035***	0.0004*
	(10.58)	(11.79)	(1.73)	(8.51)	(9.14)	(2.05)
Constant	3.9614***	−0.0553***	−0.0783***	3.6459***	−0.2353***	−0.1990***
	(277.23)	(−4.69)	(−9.51)	(132.18)	(−8.81)	(−10.59)
Time dummies	YES	YES	YES	YES	YES	YES
Region dummies	NO	NO	NO	YES	YES	YES
Industry dummies	NO	NO	NO	YES	YES	YES
Observations	2,845,770	2,845,770	2,845,770	2,845,770	2,845,770	2,845,770
R^2	0.136	0.041	0.031	0.263	0.215	0.054

Notes: ***, **, * denotes significance at the 0.1%, 1%, 5% level respectively. Robust t-values in parentheses. Injury risk is aggregated on (plant size)x(region)x(industry) cells for firms with less than 50 observations within the observation period. For larger firms the injury risk is aggregated on the firm level. There are 11,134 different risk relevant categories.
Source: Own calculations based on ASSD and AUVA.

qualitatively. Just like before, the risk-coefficient in the firm-wage regressions is of similar magnitude as the one in the standard OLS-regression whereas the worker-wage regressions reveals a much weaker relationship. In this sense, the result that most of the variation between wages and risks is captured by wage-differences between firms seems confirmed. In quantitative terms, the coefficients are now somewhat lower with an implied value of a statistical injury of about 10,000 Euros ($14,000).

In sum, the result that the firm-wage but not the worker wage are closely associated with workplace risks is consistent with the theory of compensating wage differentials. Quantitatively, the risk-coefficients on the order of 0.0035 to 0.0056 in the firm-effect regression imply an estimate of the value of a statistical injury of about 9,000 to 14,000 Euros ($13,000 to $20,000). We do not find a strong relationship between a worker's ability (as measured by the worker wage effect) and the choice of riskiness of one's work environment. The small (but significant) positive correlation between the worker wage-effect and the workplace injury risk does not indicate support for the hypothesis that high-wage workers choose safer jobs.

Firm risk, worker risk, and sorting Our linked firm-worker data set allows us to go one step further in exploring the sorting mechanism. As our data lets us construct the sequence of a worker's accidents, we can identify which worker suffered a workplace injury at which firm. Hence we can decompose the incidence of observed injuries into an effect that is attributable to the firm and an effect that is attributable to the worker. To identify the risks attached to the firm ("firm-risk") and risks associated with the characteristics or the behavior of workers ("worker-risk") we use the same procedure as we applied in the decomposition of wages (see last section).

One reason why we do not find that richer workers choose safer workplaces could be that workers differ in their attitudes towards risk (and/or their ability to cope with risk). In that case, sorting of high-wage workers to low-risk jobs occurs along with a sorting mechanism that to risky workplaces of workers with respect to the workers risk-relevant behavior. When high-wage workers are less risk-averse and/or are more able to cope with risks, the sorting of workers along the productivity-dimension competes with the sorting of workers along the risk-dimension. In fact, if sorting along the risk-dimension is the dominant one we may find that high-risk (and high-wage) workers are found in the more risky jobs.

To explore the sorting issue further we now make use also of the estimated risk effects for firms and workers. The regressions in table 3.5 now include two risk variables: one that indicates the risk attributable to the firm and another one that indicates the risk attributable to the worker. The risk-indicators for larger firms are identical to the estimated firm-risk effect. The risk-indicators for smaller firms are aggregated to the industry-region cells. The estimated worker-risk effects are not meaningful at the individual level and are therefore aggregated to the firm/labor market cell in which the worker is employed.

In column 1 we again use the log wage as the dependent variable. It turns out that the firm-risk is positively correlated with the observed wage. Moreover, we also find that the worker-risk component is positively correlated with the worker's wage. Column 2 uses the firm-wage as the dependent variable. We find that there is a positive and significant relationship between firm-wage and firm-risk. The two variables represent the building block of the theory of compensating wage differentials. We find that the two variables are positively and significantly correlated and of similar magnitude as the risk-coefficient in the simple hedonic OLS wage regression of table 3.3 and the OLS regression in column 1 of table 3.5. The firm-wage regressions of column 5 also include industry and region effects. While this yields qualitatively very similar findings as in column 2, the point estimate is now somewhat smaller. The firm-risk coefficients of columns 2 and 5 of 0.0053 and 0.0035 imply a statistical value of a workplace injury of similar magnitude as estimated before (9,000-13,000 Euros / \$13,000-\$18,000).

Interestingly, columns 2 and 5 in table 3.5 also reveal that there is a positive and significant relation between worker-risk and firm-wage. While there is no a priori restriction on the sign of this coefficient, our finding indicates that high-risk workers sort themselves into high-wage firms. Notice that our results indicate a positive relation between worker-risk and firm-wage *holding the firm-risk constant*. One possible interpretation is that firms pay high wages not only to compensate for injury risks on the job but also for other disamenities. Hence some firms pay better than others even when workplace risks are held constant. If workers willing to take risky jobs are also willing to accept other disamenities (and be compensated for them) worker-risks and firm-wages should be positively correlated. This is exactly what we see in columns 2 and 5 of table 3.5. The positive correlation between worker-risks and firm-wages turns out to be quite stable. Notice further that this sorting effect is of roughly the same magnitude as the compensating differential with point estimates of 0.0053 and 0.0035 in columns 2 and 5, respectively.

Columns 3 and 6 of table 3.5 present the results from regressions when the worker-wage effect is used as the dependent variable. Similar to the worker-wage regressions of table 3.4, the firm- and worker-risk coefficients in these regressions should be interpreted as conditional correlations (rather than as an effect of risks on wages). When workers with a higher earnings capacity select themselves into safer workplaces we should find a negative correlation between the worker-wage and the firm-risk. The results in columns 3 and 6 do not support this prediction. The respective coefficients are positive and significant, albeit of a magnitude much smaller than the coefficients in the firm-wage regressions. Similarly, we find a positive (but quantitatively small) relationship between the worker-wage and worker-risk. Taken at face value, this means that high-productivity workers are willing to take slightly more risk than low-productivity workers.

Table 3.5: Effect of firm- and worker risk component (disaggregated) on wage component

Dependent variable	ln(wage)	firm wage component	worker wage component	ln(wage)	firm wage component	worker wage component
Firm risk component	0.0058***	0.0053***	0.0004	0.0040***	0.0035***	0.0005*
	(10.77)	(11.92)	(1.91)	(8.74)	(9.31)	(2.16)
Worker risk component	0.0055***	0.0051***	0.0004	0.0039***	0.0034***	0.0005
	(9.91)	(10.90)	(1.73)	(8.04)	(8.49)	(1.89)
Time dummies	YES	YES	YES	YES	YES	YES
Region dummies	NO	NO	NO	YES	YES	YES
Industry dummies	NO	NO	NO	YES	YES	YES
Observations	2,845,770	2,845,770	2,845,770	2,845,770	2,845,770	2,845,770
R^2	0.137	0.042	0.031	0.264	0.216	0.054

Notes: ***, **, * denotes significance at the 0.1%, 1%, 5% level respectively. Robust t-values in parentheses. Injury risk is aggregated on (plant size)x(region)x(industry) cells for firms with less than 50 observations within the observation period. For larger firms the injury risk is aggregated on the firm level. There are 11,134 different risk relevant categories.
Source: Own calculations based on ASSD and AUVA.

3.7.2 Sensitivity Analysis

In this section we explore the sensitivity of our results. In particular, we look at the robustness of our results once we use a different risk measure (injury duration rather than injury incidence); we explore whether compensating differentials and sorting mechanisms vary across relevant segments of the labor market (industry and firm size); and whether the level of aggregation of the various decomposed risk measures matters for our results.

Injury duration versus incidence Injury incidence may be a weak indicator as it does not take account of the severity of a workplace accident (Hamermesh and Wolfe, 1990). Injury duration may be a better measure as the time-off work caused by an injury is a good proxy for the severity of a workplace accident. In table 3.6, we present results from regressions similar to those in table 3.5, using injury duration (the time-off work induced by a workplace accident) as the relevant risk indicator. In line with the format of previous tables, columns 1 and 4 of table 3.6 use the log wage as the dependent variable, columns 2 and 3 (and columns 5 and 6) report regression results using, respectively, the estimated firm-specific (and the estimated worker-specific) component of the wage as the dependent variable.

It turns out that using time-off work induced by a workplace accident as the relevant risk measure leads to a qualitatively similar picture. Column 2 shows that the firm-wage is positively related to injury duration attributable to the firm; and that it is also positively related (with similar magnitude) to the injury duration attributable to the worker. The extended model (column 5) yields a qualitatively very similar picture. As with the incidence variables, using injury durations as explanatory variables leads to quantitatively somewhat smaller point estimates when industry and region effects are allowed for.

A similar picture emerges with respect to the worker wage regressions (columns 4 and 6). Both the firm- and the worker-injury duration are positively associated with the worker's wage corroborating the result that sorting is not driven by higher demand for workplace safety among workers with higher earnings capacity. In contrast, we find a positive (albeit barely significant) correlation between firm-risk and worker-wage. Similarly, the coefficient of worker-risk is positive. However, in contrast to regressions using the injury-incidence variables, the worker-injury indicators are no longer significant, indicating absence of any significant correlation between worker-risk and worker-wage.

In sum, our main results turn out to be not particularly sensitive to the risk measure used in the hedonic wage regression.

Compensating differentials by industry and firm size Industries are not identical with respect to the risk of a workplace injury. The construction industry is perhaps a special case because a large fraction (about 25 percent of all male blue collar workers)

Table 3.6: Injury duration (disaggregated) on wage component

Dependent variable	ln(wage)	firm wage component	worker wage component	ln(wage)	firm wage component	worker wage component
Firm component of injury duration	0.00032***	0.00030***	0.00001	0.00020***	0.00018***	0.00002
	(9.88)	(11.28)	(0.92)	(7.46)	(8.10)	(1.68)
Worker component of injury duration	0.00032***	0.00030***	0.00001	0.00020***	0.00018***	0.00002
	(9.58)	(10.93)	(0.78)	(7.23)	(7.92)	(1.31)
Time dummies	YES	YES	YES	YES	YES	YES
Region dummies	NO	NO	NO	YES	YES	YES
Industry dummies	NO	NO	NO	YES	YES	YES
Observations	2,845,770	2,845,770	2,845,770	2,845,770	2,845,770	2,845,770
R^2	0.133	0.037	0.031	0.262	0.213	0.054

Notes: ***, **, * denotes significance at the 0.1%, 1%, 5% level respectively. Robust t-values in parentheses. Injury risk is aggregated on (plant size)x(region)x(industry) cells for firms with less than 50 observations within the observation period. For larger firms the injury risk is aggregated on the firm level. There are 11,134 different risk relevant categories.

Source: Own calculations based on ASSD and AUVA.

is employed in this industry and because the injury risk in the construction industry is disproportionately high. For this reason, it may well be that risk in the construction industry plays a larger role in the compensation than in other industries. Table 3.7 explores this issue. In column 1, we present the risk-coefficients when the sample is restricted to workers and firms in the construction industry; in column 2, we present the corresponding coefficients from a sample based on non-construction workers and firms. Notice that the coefficients in this table are comparable to the coefficients in columns 4-6 of table 3.5, as region- (and, for the non-construction sample, also industry-) dummies are included in the regressions.

Table 3.7 confirms that the compensation for injury risks in the construction industry is different from the non-construction industries. In the *firm-wage* regressions (panel B), we find that the coefficient of firm-risk is 0.0047 in the sample of construction workers but only 0.003 in the sample of non-construction workers. This implies that the value of statistical injury is roughly 30 percent higher in the construction industry than economy-wide. This is to some extent due to the fact that a typical workplace injury in the construction industry is more severe (i.e. lasts roughly 10 percent longer) than a typical workplace injury in other industries. Notice also that in two respects the finding both in construction and non-construction industries confirm our previous estimates. First, the firm-risk effect on firm wages is almost identical as in the standard OLS equation confirming that the cross-sectional relationship between workplace injuries and wages is largely driven by compensation for injury risks. Second, we also find that the correlations between wage-effects and risk-effects are quite similar as in the previous equations. In particular, our results are consistent with the same mechanisms that sorts high-risk workers into high-wage firms as in our previous results. In particular, we find that high-risk workers are found in high-wage firms: this suggests that high-risk (construction and non-construction) workers are also willing to accept other workplace disamenities (and be compensated for them).

In the *worker-wage* regressions (panel C) we find that the conditional correlation between firm-risk and worker-wage and between worker-risk and worker-wage is significantly positive in the construction industry but absent in non-construction industries. One reason for this result could be that the higher prevalence of injury risks in the construction industry implies a higher importance of injury risk considerations in job- and mobility-choices. In contrast, in non-construction industries workplace (dis)amenities (other than injury risks) play a relatively more important role so that the sorting of workers across workplaces is strongest driven by considerations unrelated to the risk of workplace injuries.

A related issue is whether the compensation of workplace injuries is similar across large and small firms. It is a well established fact that larger firms pay better than smaller firms but the reason for this size-differential is rather controversial. An important

Table 3.7: Wages and injury risk, construction vs. non construction

		construction	non construction
A. ln(wage rate)			
	Firm risk component	0.0064***	0.0032***
		(11.25)	(5.64)
	Worker risk component	0.0062***	0.0031***
		(10.70)	(5.19)
	R^2	0.111	0.287
B. Firm wage component			
	Firm risk component	0.0047***	0.0031***
		(10.62)	(6.44)
	Worker risk component	0.0046***	0.0030***
		(9.63)	(5.94)
	R^2	0.074	0.230
C. Worker wage component			
	Firm risk component	0.0016***	0.0001
		(5.15)	(0.43)
	Worker risk component	0.0016***	0.0001
		(4.80)	(0.31)
	R^2	0.021	0.063
Time dummies		YES	YES
Region dummies		YES	YES
Industry dummies		-	YES
Observations		648,676	2,197,094

Notes: ***, **, * denotes significance at the 0.1%, 1%, 5% level respectively. Robust t-values in parentheses. Injury risk is aggregated on (plant size)x(region)x(industry) cells for firms with less than 50 observations within the observation period. For larger firms the injury risk is aggregated on the firm level. There are 11,134 different risk relevant categories.
Source: Own calculations based on ASSD and AUVA.

hypothesis states that problems of asymmetric information are more prevalent in larger firms and hence compensation is to a larger extent driven by wage policy mechanisms that try to solve incentive problems arising from these information asymmetries. Inter alia, this may lead to labor market equilibria where utilities across jobs are no longer equalized and the strict relationship between workplace risks and wages, that emerges under perfect labor markets, does no longer emerge with labor market imperfections. In other words, compensation for workplace (dis)amenities play a relatively more important role in smaller firms where the market forces have more direct impact on the determination of earnings. A similar argument, perhaps equally important in the Austrian context, implies that wages in larger firms are stronger determined by union power and rent sharing than in smaller firms where workers are less strongly unionized (or where the threat of unionization is less prevalent). While the presence of unions does not imply that compensation for workplace amenities becomes irrelevant (Duncan and Stafford, 1980), union power and the resulting changes in labor relations within the firm may alter firm wage policies in ways that dominate the importance of compensation of workplace risks.

Table 3.8 divides the sample into small and large firms. More precisely, the sample is split up into firms with less than 500 employees (about 80 percent of observations) and firms with more than 500 employees (20 percent of observations). In line with the above arguments, the results show that compensation for workplace injuries is confined to smaller firms. The coefficients in the small-firm sample are very similar to the results in table 3.5 (extended model, column 4-6) both with respect to the compensating differential (effect of firm-risk in the firm-wage equation) and with respect to sorting (the remaining coefficients in panels A and B). In contrast, the coefficients of all risk-variables are insignificant in the large-firm sample.

Aggregation of risk measures As a final sensitivity test, we look at the relevance of possible measurement errors that may result by construction of our risk indicators. As mentioned above, both worker- and firm-risk variables are measured at the firm-level for larger firms (with 50 employment observations or more) while these measures are proxied by industry-region labor market cells for smaller firms (with less than 50 employment observations). Although this variable has the advantage of processing the detailed information on individual (firm and worker) injury incidence, it may still be too noisy calling for a higher aggregation level. Alternatively, aggregating to industry-region market has some element of arbitrariness and disregards (potentially relevant) information that would be available at more disaggregated levels. This means we need to check how sensitive our results are with respect to the aggregation level at which we measure the risk-exposure and the risk-behavior of workers. In theory, the appropriate level of risk-exposure is the firm. In practice, using injury events as a proxy for a worker's injury risk is only reasonable for large firms. For smaller firms, the time-period over which we measure

Table 3.8: Wages and injury risk, large firms vs. small firms

		plant size < 500	plant size ≥ 500
A. ln(wage rate)			
	Firm risk component	0.0046***	−0.0012
		(13.79)	(−0.38))
	Worker risk component	0.0045***	−0.0013
		(12.92)	(−0.44)
	R^2	0.249	0.365
B. Firm wage component			
	Firm risk component	0.0039***	−0.0012
		(14.73)	(−0.53)
	Worker risk component	0.0038***	−0.0012
		(13.50)	(−0.49)
	R^2	0.207	0.594
C. Worker wage component			
	Firm risk component	0.0006***	0.0001
		(3.48)	(0.08)
	Worker risk component	0.0007***	−0.0001
		(3.34)	(−0.05)
	R^2	0.046	0.120
Time dummies		YES	YES
Region dummies		YES	YES
Industry dummies		YES	YES
Observations		2,383,335	462,435

Notes: ***, **, * denotes significance at the 0.1%, 1%, 5% level respectively. Robust t-values in parentheses. Injury risk is aggregated on (plant size)x(region)x(industry) cells for firms with less than 50 observations within the observation period. For larger firms the injury risk is aggregated on the firm level. There are 11,134 different risk relevant categories.
Source: Own calculations based on ASSD and AUVA.

injury events is simply too short. Whether or not an injury takes place is determined by bad or good luck, and actual injury outcomes may be a poor proxy for actual injury risks. In order to test, how sensitive our results are with respect to the particular risk measure, we use four additional risk measures, based on alternative aggregation levels, to proxy the worker's injury risk. Table 3.9 presents the results.

Column 1 of table 3.9 reproduces results from the highest level of aggregation, i.e. when the firm- and worker-effects are aggregated to two-digit industries leading to 36 different labor market segments (so that the risk-indicators can take one of only 36 different values); column 2 divides segments further into firm-size classes (-14, 15-49, 50-99, 100-499, 500+ employees) within industries which lets us end up with 167 labor market cells (in 13 of the $36 \cdot 5 = 180$ potential cells there are no firms); column 3 divides segments further into regions (more than 120 political districts) leading to 1,277 labor market cells; column 4 repeats the results of table 3.5 which are based on 11,134 labor market cells; finally, column 5 is based on estimated firm-specific risk measures without any further aggregation leading to 62,479 different values for the risk indicator.

From table 3.9 it becomes clear that the level of aggregation does not affect our main results qualitatively. In all firm-wage regressions of panel A the firm-risk coefficients have a positive sign and are statistically significant (albeit only at the 10 percent level when we use the very coarse industry classification of columns 1). However, we see that the aggregation level of the risk-measure affects our results quantitatively. In particular, the point estimate of firm risk in the firm-wage equation is somewhat larger than with a higher aggregation level of the risk measure (columns 1 and 2). In columns 3 to 5 we disaggregate the risk measure by region and use firm-specific information, the point-estimates become somewhat smaller. There may be two reasons for this. On the one hand, the industry classification may be too coarse which leads to an upward bias in the compensating differential (Lalive, 2003). On the other hand, the firm-classification may be too fine, so that for small firms observed injury incidences are not informative on the true injury risk. In other words, for small firms the firm-specific, which is flawed by measurement error and the risk-coefficient, is biased towards zero. The firm-risk coefficient in the firm-wage equation is 0.002 when the risk-indicator is measured at the firm-level (column 5) and is indeed the smallest among all coefficients in table 3.9. With higher aggregation levels it is between 0.0035 (firm-level classification for larger firms, industry-size region cells for smaller firms, column 4) and 0.006 (industry-size-region classification, column 3). Our preferred estimate where firm-specific risk measures are used for large firms and the auxiliary industry-size-region segments are used only for smaller firms produces an estimate of 0.0035. For obvious reasons, the measurement bias is of minor importance when risk is measured at the industry level whereas aggregation bias is zero when risk is measured at the firm-level. Hence we conclude that our intermediate measure where we avoid aggregation bias by exploiting firm information and at the same time avoid

Table 3.9: Wages and injury risk, different aggregation levels

Aggregation level	Industry	Ind. * FS	Ind*FS*region	Ind*FS*region	Firm
Number of categories	36	167	1,277	11,134	62,479
A. Firm wage component					
Firm risk component	0.0123	0.0152***	0.0061***	0.0035***	0.0021***
	(1.76)	(5.00)	(5.30)	(9.31)	(10.77)
Worker risk component	−0.0050	0.0084	0.0057***	0.0034***	0.0020***
	(−0.49)	(1.78)	(4.58)	(8.49)	(9.97)
R^2	0.091	0.229	0.217	0.216	0.215
B. Worker wage component					
Firm risk component	−0.0021	0.0011	0.0003	0.0005*	0.0004**
	(−0.95)	(1.04)	(0.59)	(2.47)	(3.21)
Worker risk component	−0.0047	0.0008	0.0000	0.0005	0.0004**
	(−0.94)	(0.60)	(−0.20)	(2.12)	(2.93)
R^2	0.031	0.054	0.054	0.054	0.054
Time dummies	YES	YES	YES	YES	YES
Region dummies	NO	YES	YES	YES	YES
Industry dummies	NO	YES	YES	YES	YES
Observations	2,845,770	2,845,770	2,845,770	2,845,770	2,845,770

Notes: ***, **, * denotes significance at the 0.1%, 1%, 5% level respectively. Robust t-values in parentheses. Injury risk is aggregated on (plant size)x(region)x(industry) cells for firms with less than 50 observations within the observation period. For larger firms the injury risk is aggregated on the firm level. There are 11,134 different risk relevant categories.
Source: Own calculations based on ASSD and AUVA.

measurement bias by aggregating small firms to industry-size-region labor market cells should keep the bias in the estimated compensating differential small. In sum, our result that firm-wages are positively and significantly affected by firm-risk turns out robust and independent of the particular aggregation level. We consistently find a positive and significant relationship between the building blocks of the theory of compensating wage differentials: firm-risks and firm-wages.

We also consistently find a positive conditional correlation between worker-risk and firm-wages, suggesting that workers willing to take higher risks are sorted into workplaces that are associated with (and compensate) other disamenities. Only when we aggregate worker-risks to the industry level, such an effect does not materialize. Finally, irrespective of the aggregation level, the relationship between risk indicators is much smaller in the worker-wage regressions (panel B) than the risk-effects in the firm-wage equations (panel A). In particular, we find that worker-wage and firm-risk are positively correlated contradicting the hypothesis that workers with higher earnings capacity choose safer workplaces.

3.8 Conclusions

In this chapter, we have presented new evidence on compensating wage differentials for injury risks. Using linked firm-worker data not only reporting workers employment and earning histories but also the worker's histories of workplace injuries at various employers. This allows us to identify the building blocks of the theory of compensating wage differentials: the firm-specific components of wages and risk. The risk of a workplace injury results from the interplay between the risk inherent in the workplace (firm-risk) and the risk-relevant behavior for the worker (worker-risk) by which we mean the workers' willingness to take risks and/or the workers' ability to avoid risk at the workplace. The theory of compensating wage differentials states that workers subject to higher workplace injury risks should earn higher wages. In contrast, workers should not be compensated for innate risky behavior due to, for instance, low risk aversion and/or lack of dexterity, precaution, or diligence.

Our empirical strategy decomposes observed wages and workplace injuries into firm and worker components using econometric techniques developed by (Abowd *et al.*, 2002). Hence our empirical strategy has added two elements to existing studies. First, by identifying the relationship between firm-wage and workplace-injury risks it identifies the building blocks of the theory and provides empirical estimates for the hedonic wage function that go one step further than existing studies. Second, by identifying the wage- and risk-components attached to the worker, our results also shed new light on the sorting of worker-types across workplaces that differ in (dis)amenities.

Our empirical analysis produces several interesting results. *First*, our analysis based on firm- and worker-specific risk indicators lead to an estimated compensating wage differential that is quite similar to the one obtained from a standard cross-sectional hedonic wage equation. Regressing individual wages on aggregate industry (or occupational) injury risk yields an implied value of a statistical (non-fatal) injury of about $20,000. This is on the lower side of the range of $20,000 to $70,000 found in samples from other industrialized countries (Viscusi and Aldy, 2003). *Second*, the estimated compensating differential (and the estimated value of a statistical injury) remains unchanged when we focus on the building blocks of the theory. Regressing the firm-specific wage on firm-specific risks, we find a compensating wage differential that is of roughly equal size than the one obtained in the standard cross-sectional wage regression. This result turns out quite robust. We also find that our results remain unchanged when we use the expected days off work (rather the expected number of injuries) as the relevant risk indicator. We also find some heterogeneity in the compensating differential across industries. In particular, smaller firms and firms in the construction industry compensate risks stronger than average. *Third*, our empirical strategy also allows us to shed new light on the sorting of workers across firms that offer different workplace (dis)amenities. We find no evidence in favor of a sorting of high-productivity workers into low-risk jobs. This suggests that the bias of the compensating differential obtained from a standard cross-sectional hedonic wage function that is due to unobserved productivity of workers is small. However, we find that, conditional on firm-risk, high-risk worker sort themselves into high-wage jobs. This is consistent with a explanations that workers who are willing to take high risks also accept other workplace disamenities (and be compensated for them).

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3.A Appendix

This appendix discusses the estimation algorithm. The least squares estimator of β, θ and ψ solves the following normal equations

$$\begin{bmatrix} X'X & X'D & X'F \\ D'X & D'D & D'F \\ F'X & F'D & F'F \end{bmatrix} \begin{bmatrix} \beta \\ \theta \\ \psi \end{bmatrix} = \begin{bmatrix} X'y \\ D'y \\ F'y \end{bmatrix} \tag{3.5}$$

It is not possible to invert the cross-product matrix due to the large number of person and firm effects and due to computer memory constraints. In this chapter we apply a modified version of the iterative gradient method proposed in Abowd *et al.* (2002) to find the solution to the normal equations. The idea of this estimator is simple. Rearranging the system of linear equations in (3.5) yields

$$\begin{bmatrix} X'X\beta \\ D'D\theta \\ F'F\psi \end{bmatrix} = \begin{bmatrix} X'(y - D\theta - F\psi) \\ D'(y - X\beta - F\psi) \\ F'(y - X\beta - D\theta) \end{bmatrix} \tag{3.6}$$

These are four blocks of normal equations that yield the required least squares solution given the least squares solution of the remaining three sets of parameters.

The iteration protocol is as follows. Choose starting values β_0, θ_0 and ψ_0. Let l index iterations. Solve for β_l, θ_l, and ψ_l using (3.6) based on the estimate of the other parameters in iteration $l-1$. This gives the following updating rule

$$\begin{bmatrix} \beta_l \\ \theta_l \\ \psi_l \end{bmatrix} = \begin{bmatrix} [X'X]^{-1} X'(y - D\theta_{l-1} - F\psi_{l-1}) \\ [D'D]^{-1} D'(y - X\beta_l - F\psi_{l-1}) \\ [F'F]^{-1} F'(y - X\beta_l - D\theta_l) \end{bmatrix} \tag{3.7}$$

Intuitively, the current estimate of β, for instance, is found by regressing the residuals $y - D\theta_{l-1} - F\psi_{l-1}$ on the matrix X.

The algorithm is partially recursive in using the fact that the current value of β, β_l can already be used in estimating θ_l. In estimating ψ_l, the current values of β_l and θ_l are used to form the residuals, etc. The algorithm converges to the true least squares solution because parameter updates are chosen to fulfill the normal equations given the values of the other parameters. We determine convergence to be achieved when the absolute change in the sum of squared errors between iteration l and $l-1$ falls below $1 \cdot 10^{-11}$.

Moreover, Abowd *et al.* (2002) show that it is necessary to identify connected groups of firms and workers in the data set. A connected group is defined as the set of firms and workers such that every worker in the set is connected to every other worker in the set by at least one move (either directly or indirectly) between their respective employers. Within a connected group, the model identifies all worker effects and firm effects up to one effect in each dimension. In the empirical analysis we focus on the largest group (group 1) which covers more than 85 % of all observations, more than 85 % of all workers, and more than 55 % of all firms. (Smaller groups typically consist of one worker being employed with the same one person firm in the entire sample period.) We normalize all effects such that they can be interpreted as the deviation of the firm effect from the average firm effect in the group.

We apply this procedure separately, to the log of daily earnings w_{it}, and the risk of an injury or illness, R_{it}.

CHAPTER 4

Compensating Wage Differentials & the Value of a Statistical Injury

joint with Andreas Kuhn

"The value of life is not in the length of days,
but in the use we make of them;
a man may live long yet very little."

Michel de Montaigne (1533–1592), French writer and philosopher

4.1 Introduction

It has long been recognized in economics that differences in wages are not only due to the fact that individuals differ in their productivity–relevant characteristics (e.g. education), but also due to the fact that the jobs offered to workers differ enormously along various dimensions (workplace safety being only one important example). Workers presumably not only value the monetary payoff from working, but also the non–monetary characteristics, potentially giving rise to compensating wage differentials (Rosen, 1986). This means firms offering jobs with "negative" characteristics, that is, attributes to which workers attach a negative value, must attract workers by paying them higher wages ceteris paribus, thus "compensating" them for the negative aspects of the job (and vice versa for "positive" workplace characteristics). Non–monetary characteristics of jobs are of course manifold, most empirical studies though focus on workplace safety, that is on the risk compensation for both fatal and non–fatal accidents (e.g. Viscusi and Aldy, 2003).

The theory of compensating wage differentials has inspired a huge number of empirical studies trying to pin down the compensation for undesirable workplace attributes. Due to the implicit trade–off between job amenities and wages, observed (or rather, estimated) compensating wage differentials can be used to assess the value of a statistical life or injury, respectively. These empirical results in turn may directly influence public policy, since cost–benefit analyses with respect to safety regulations need empirical assessments on the monetary value of such regulations (this applies not only to regulations of safety at the workplace, but also to environmental regulations for example).

Yet, the intuitive appeal of the theory notwithstanding, empirical studies face some fundamental problems concerning the identification of compensating wage differentials. The main problem is rooted in unobserved productivity differences between individuals and the thereupon based sorting of workers into jobs with different risks (due to the positive income elasticity of the value of a statistical accident). This presumably explains the rather large variation in the estimated compensation for risks on the one hand, but also the fact that many empirical studies report no compensation for risk or even report compensating wage differentials having the "wrong" sign (at least with respect to non–fatal injury risks). For example, the survey by Viscusi and Aldy (2003) reports a rather wide range of estimates for the value of a statistical injury from about $20,000 to $70,000 (for the United States only).

This chapter presents empirical evidence on the compensation for non–fatal accident risk in Switzerland, using a data set compiled from two different sources (which we will discuss in detail below). Our study has three main features. *First*, we will exclusively focus on non–fatal accidents. This focus reflects the fact that most accidents have (fortunately) non–fatal consequences and thus, from the viewpoint of public health and safety, merit the most attention.[1] In the year 2004 (the year of our empirical analysis), for example, the Swiss Accident Insurance Fund reports about 246,000 non–fatal accidents related to work but only 188 fatal accidents. *Second*, we observe the number of non–fatal accidents not only within entire industries, but also within cells defined by industry$\times$skill–level of the job. This is a tremendous advantage from an empirical point of view, since risks at (too) high levels of aggregation mix the risks of very different groups of workers and different willingness to pay for avoiding risk, which might lead to biased estimation of the compensation for risk in the workplace. *Third*, we capitalize on the availability of longitudinal wage information, which allows us to use simple panel estimation methods in order to isolate the firm wage component. We believe that our empirical approach, on the one hand using the number of non–fatal accidents within narrower cells than usually available, and on the other hand combining panel data estimation methods with simple non–parametric stratification, transcends the typical hedonic wage function approach

[1] Our focus though is also due to the available data on non–fatal accidents as well as the empirical approach we take, as we will discuss in detail in section 4.3 and section 4.4 below.

often used in the literature on the subject.

The main findings of our empirical analysis are the following. *First*, we find that a simple hedonic wage regression, where the observed log wage is regressed on the risk measure (and additional control variables) yields a compensation for non–fatal accident risk which is statistically zero, a result that is in line with some previous empirical studies. The leading explanation for this result (which runs counter to theory) is presumably the sorting of workers which differ in their unobserved productivity. *Second*, moving on to, in a sense, more sophisticated (but, we believe, in this case also more reliable) methods, we find a positive point estimate for the compensation of non–fatal accident risk. Our preferred point estimate yields an implicit value of a statistical injury of about 40,000 Swiss francs (which lies well within the range given by studies from the U.S. labor market, as well as from studies outside the U.S.). On the other hand, using different estimation methods yields considerably different values for the value of a statistical injury. As we will discuss later, a significant cause of this wide range of estimates is the difference in the estimation methods used. *Third*, comparing the different estimation methods may shed some light on the problem of endogenous sorting of workers into jobs with different risks, which presumably yields biased estimates for the compensation of risk. Our results are in fact in line with the argument supported by Hwang *et al.* (1992), among others, that such endogenous sorting gives rise to severe *underestimation* of risk compensation. *Fourth*, we find significant differences between men and women on the one hand, and between smaller and larger firms on the other hand with respect to the compensation of non–fatal accident risk. *Fifth* and finally, our results also show that the kind of risk–data available can make an important difference for the empirical assessment of risk compensation.

The rest of this chapter is organized as follows. We start with a discussion of the relevant literature on compensating wage differentials, focusing on empirical studies estimating the compensation for non–fatal accident risk. In section 4.3, we discuss the two data sources we rely on, discuss the construction of the variables of main interest – along with some descriptive statistics. We then expore issues of identification and estimation in section 4.4. Specifically, we will discuss three different approaches to identification and estimation. We start with a simple hedonic wage regression model, where the wage is simply regressed on individual– and firm–specific characteristics. The second approach is based on the idea that we can control for unobserved heterogeneity of individuals by appropriately stratifying the sample. The third approach we take capitalizes on the longitudinal structure of the wage data. We isolate the wage component, which is specific to the firm and then use only this part of the wage to estimate risk compensation. The results of the different estimation methods are presented and discussed in section 4.4. Based on our econometric results, we further present estimates of the value of a statistical (non–fatal) accident in Switzerland. Section 4.6 concludes.

4.2 Related Literature

4.2.1 Compensation for Workplace Accident Risk

There is a large number of empirical studies which try to pin down the compensation for accident risks, as well as for a wide range of other job amenities and disamenities (the surveys by Viscusi and Aldy (2003) and Viscusi (1993) are of special interest here; see also the more recent, but less thorough survey by Ashenfelter (2006)).[2] Most empirical studies find a positive compensation for fatal accident risk, often yielding high implicit values of a statistical life. For example, Viscusi and Aldy (2003) report that half of the studies from the U.S. labor market surveyed in their article give a value of a statistical life within the range of \$3.8–\$9.0 million (in 2000 dollars), the median estimate being about \$7 million. Most studies from outside the U.S. labor market give estimates within the same range. It is difficult to assess the exact reasons for this wide range of estimates, since the studies differ in various ways, for example with respect to the available data and risk measure[3], or in the econometric methods applied.

The evidence on the compensation for non–fatal accident risk is much less coherent, which is somewhat surprising since most studies that present estimates of such compensation are based on the same data as estimates for the compensation for fatal accident risk. Viscusi and Aldy (2003) report, for both the U.S. as well as other labor markets, a probable range for the value of a statistical injury of about \$20,000–\$70,000 per injury.

4.2.2 Endogenous Sorting

The main problem from the empirical point of view is the potential sorting of workers into jobs differing in their risk of accidents. Hwang *et al.* (1992), among others, argue that the problem of main concern are differences in unobservables which in turn relate to the productivity of workers and thus may lead to sorting of workers into jobs with different risks. The sorting of workers in turn is endogenous due to the fact that the income elasticity of the value of a statistical life or injury is positive, i.e. more productive workers sort themselves into less risky jobs by accepting ceteris paribus lower wages. Viscusi and Aldy (2003), for example, report an income elasticity of about 0.5–0.6. On the other hand though, Shogren and Stamland (2002) argue that the bias in estimating the compensating wage differential could run in the other direction, assuming that workers not only differ

[2]Compensating wage differentials have also been found, for example, for the risk of unemployment (Moretti, 2000; Lalive *et al.*, 2006), for shift work (Kostiuk, 1990), and uncertainty with respect to future earnings (Feinberg, 1981).

[3]Most importantly perhaps, some studies rely not on direct measures of risk (i.e. number of accidents), but base their analyses on tradeoffs outside the labor market, e.g. on the tradeoff between traffic accidents and the price of automobiles (Dreyfus and Viscusi, 1995) or fatalities related to bicycle accidents and the prize of bicycle helmets (Jenkins *et al.*, 2001). Other studies have used subjective assessments of risk, as for example Viscusi and O'Connor (1984) and Viscusi and Hersch (2001).

in their productivity, but also with respect to their skill in avoiding accidents. Thus, workers in risky jobs could be either more tolerant to risk or more skilled in avoiding risk (or both). Thus they show that the estimated risk compensation might actually be upward biased, rather than downward biased.

Some studies have tried to approach the problem of endogenous sorting by using instrumental variables (Garen, 1988; DeLeire and Levy, 2004, for example,). The study by Garen (1988), for example, tries to correct for the endogeneity of job risk by using a system of simultaneous equations where marital status and the number of dependents are used as instruments for the preference over risk.

4.2.3 Measurement of Risk

Another empirical issue concerns the measurement of the risk of an accident. First, as pointed out by Mellow and Sider (1983) for example, typical survey data are more often than not plagued by measurement error, i.e. it seems to be the case that workers often misreport their industry affiliation and/or their exact occupation. Assuming that this kind of measurement error is random, this causes the compensating differential to be biased towards zero. Second, there clearly is a trade–off of the following form. On the one hand, risk measurements at a low level of aggregation are preferred, as otherwise one might mix workers with very different occupations into the same risk categories. On the other hand though, risk measures at a low aggregation level run into the problem that many cells will have zero risk, at least for shorter periods of time. This is specifically true for fatal accident risk, yet obviously also applies to non–fatal injuries.

4.2.4 Estimation

The most prevalent approach in the empirical literature is via estimation of hedonic wage functions, that is, by running regressions of the wage on characteristics of both the workers *and* jobs. As we will make explicit in section 4.4, this approach is likely to fail identification because it is unlikely that this approach can effectively deal with the problem of endogenous sorting of workers into jobs (as pointed out above, some studies have tried to instrument endogenous sorting by using family characteristics).

As we will discuss in detail in section 4.4 below, our empirical approach of choice relies on the panel structure of the wage data. Thus, our study also relates to work on matched employer–employee data (e.g. Abowd and Kramarz, 1999) as well as the panel data estimation methods in general (e.g. Wooldridge, 2002). We cannot directly apply the methods of Abowd and Kramarz (1999) though, because our wage data has a longitudinal structure only with respect to the employer, but not with respect to the individual worker.

4.2.5 Empirical Evidence for Switzerland

To the best of our knowledge, there is only a single published study on the compensation of accident risk for Switzerland by Baranzini and Ferro-Luzzi (2001), focusing on fatal accident risk only. They report estimates for the value of a statistical life ranging from about 12 to about 32 million Swiss francs. Besides having a different focus (our focus in on non–fatal accident risk only), our study differs in at least two further ways. *First*, we have access to the number of non–fatal accidents not only within industries, but within narrower cells, defined over industry×skill–level of the job. *Second*, we do not primarily and exclusively rely on simple hedonic wage regressions for the estimation of risk compensation, instead we use the longitudinal structure of the wage information in a first stage in order to deal with the endogenous sorting of workers.

4.3 Data

4.3.1 Data Sources

We use two different data sources. The first data source is the Swiss Wage Structure Survey (SWSS; "Lohnstrukturerhebung (LSE)"), which is a biannual survey among firms which is administered and made available by the Swiss Federal Statistical Office. The SWSS is one of the two largest official surveys in Switzerland focused mainly on employment–relevant information.[4] The SWSS is a survey of firms, covering the population of large firms along with a random sample of small firms. We use three different waves of the SWSS (from the years 2000, 2002, and 2004) and we extract individual monthly earnings along with several individual–specific characteristics (see section 4.3.4 below) on details.

Our risk measure corresponds to the number of non–fatal accidents within cells defined over industry (forty different industries on a two–digit level) and skill–level of the job (four different levels). The data have been provided by the Swiss Accident Insurance Fund (SAIF; "Schweizerische Unfallversicherungsanstalt (Suva)"), which is the most important accident insurance fund in Switzerland. The number of non–fatal accidents within industry×skill–level cells are available for the year 2004.

[4]The second important labor market survey is the Swiss Labor Force Survey (SLFS; "Schweizerische Arbeitskräfteerhebung (SAKE)"). The two main advantages of using the SWSS over the SLFS are the following: First, the SWSS allows isolating the wage firm fixed effect, which is the part of the observed wage where risk compensation should show up. Second, the SWSS is (opposed to the SLFS) mailed to employers, and thus misclassification of occupations and industries should only be of minimal order (the same is arguably true for wages).

4.3.2 Definitions

One of the main features of our analysis is that our risk measure r_k gives the number of non–fatal accidents per year and per 1,000 workers within a given industry×skill–level cell k (instead of within–industry only). Data on the absolute number of non–fatal accidents for the year 2004 is available within cells defined over industry×skill–level of job. Now, because the SAIF does not directly have the number of workers within these cells and because workers are not uniformly distributed over these cells, we also need to know the distribution of workers over these cells in order to compute the risk of a non–fatal accident. To this end, we simply use the distribution of workers in the SWSS (from the year 2004), and then approximate the population distribution of workers by multiplying the number of workers within a given cell with the total number of workers which are covered by the SAIF (about 1.827 millions in the year 2004).

Note that there is a fundamental trade–off with respect to the risk measure chosen: On the one hand, risk measures on a highly disaggregated level are preferred, such that we do not pool accident risks of individuals working in very different occupations and jobs. This has been pointed at, for example, by Viscusi (1993, p.1928), noting that "[t]he main deficiency of industry–based data is that they pertain to industry–wide averages and do not distinguish among the different jobs within that industry [...]". On the other hand, accidents observed at a very low level of aggregation also give rise to estimation problems, because the number of accidents tends towards zero for most cells if we shrink the size of the risk–relevant cells. That, in fact, is the reason why we decided not to use the information about fatal accidents for this study. Disaggregating the number of fatal accidents over the skill–level of job actually yields far too many cells with zero number of accidents.

The SWSS includes average gross monthly wages for full–time employment (i.e. 172 hours per month), including mandatory social security contributions and extra pay (e.g. for night work, 13. monthly wage). The SWSS also includes several socio–demographic characteristics (e.g. age, gender, tenure, educational attainment (highest degree), citizenship), but also different firm characteristics (most importantly, the size of the firm along with the geographic location).

4.3.3 Measurement Error

One main advantage of our data is that measurement error in the risk data and industry–affiliation of workers is arguably of minor significance (as already mentioned in section 4.2, Mellow and Sider (1983) have pointed out the problem of misclassification of both industry and occupation). This is important because measurement error in the risk variable tends to bias the compensating wage differential towards zero (measurement error in the dependent variable (i.e. wage) is, of course, also common but of less concern). We are confident that the measurement error for both our risk measure and industry–affiliation is of no great importance, since the SWSS does not involve employees but obtains the data from the employer directly (such that misclassification of either industry and/or occupation is unlikely to occur). For the same reason, we also believe that our wage information is more reliable than the information available in typical survey data (although presumably less reliable than administrative data). Additionally, our risk measure is directly obtained from administrative sources and should thus cover all relevant accidents.

4.3.4 Descriptive Statistics

Table 4.1 shows descriptive statistics for both the overall sample as well as the sample of individuals in jobs of the lowest skill–level (that will be used in the empirical analysis discussed below). In both samples, we only consider workers aged between 16 and 64 (for men) and between 16 and 61 (for women). A second restriction applies to the size of the employer. Because we are estimating wage fixed effects for each firm, we also restrict the sample to workers from firms which have at least ten workers in each of the four job skill–levels in each year. The overall sample includes more than one million individual workers, the subsample of workers in the lowest skill–level (with respect to the job, *not* with respect to the educational attainment of the worker) consists of about 300,000 individual workers. In both cases, there are about 3,500 different firms (due to the restriction on firms). As we will discuss in–depth in section 4.4 below, our preferred estimation approach will focus exclusively on workers within a given skill–level as collected in the SWSS, as we believe that such a stratification of the workers yields more reliable estimates of the compensating wage differential.

We begin with describing the overall sample, which is representative of the Swiss labor market as a whole. The typical worker in the Swiss labor market has gross earnings equal to 6,300 Swiss francs a month, is about 40 years old and has about 9.5 years of tenure and is more likely to be a man. The average employer has more than 2,800 workers (reflecting the sampling structure of the SWSS as well as the restriction with respect to the selection of the employers). About two thirds of the workers are married, the other third single. The distribution of workers with respect to educational attainment highlights two important characteristics of the Swiss labor market in terms of education. First, compared to other countries, the number of workers with tertiary education is rather low (e.g. only about 5.5% of the workers have a university degree). Second, about half of the workers hold a vocational training. Another important characteristic of the Swiss labor market is the large fraction (about 20%) of workers without Swiss citizenship.

Focusing on individuals working in jobs with the lowest skill–level (columns 3 and 4 of table 4.1) yields the expected result that some groups are overrepresented in the analysis sample relative to the overall sample of individuals (although this subset of individuals is similar to the overall sample with respect to some characteristics, for example age and

Table 4.1: Descriptive statistics

	Skill-level 1		Skill-level 1-4	
	Mean	Std. Dev.	Mean	Std. Dev.
Monthly wage	4526.63	1069.26	6371.88	3466.72
Natural logarithm of monthly wage	8.39	0.23	8.68	0.38
Non fatal accident risk (per 1,000 workers)	45.40	59.13	93.01	150.42
Age	40.19	11.66	40.71	11.14
Female	0.54	0.50	0.42	0.49
Tenure	7.63	8.18	9.06	9.12
Size of the firm	2714.94	7820.84	3108.01	7890.73
Marital status				
Single	0.27	0.44	0.32	0.47
Married	0.62	0.49	0.58	0.49
Others	0.11	0.31	0.10	0.30
Education				
University degree	0.00	0.05	0.06	0.23
College of higher education	0.00	0.05	0.05	0.21
Higher professional degree	0.01	0.08	0.07	0.26
Teachers' certificate	0.00	0.04	0.00	0.07
High School	0.01	0.11	0.02	0.14
Finished professional education	0.27	0.45	0.50	0.50
Firm intern professional education	0.14	0.34	0.07	0.25
Secondary school	0.48	0.50	0.18	0.38
Other degree	0.08	0.28	0.05	0.22
Citizenship				
Swiss citizenship	0.52	0.50	0.68	0.47
Short tem residence authorization	0.01	0.11	0.01	0.08
Long term residence authorization	0.08	0.28	0.05	0.23
Permanent recidence permit	0.29	0.45	0.17	0.37
Cross-border commuter	0.06	0.24	0.07	0.25
Others	0.03	0.18	0.03	0.16
Geographic region				
VD, VS, GE	0.19	0.39	0.16	0.37
BE, FR, SO, NE, JU	0.23	0.42	0.21	0.41
BS, BL, AG	0.12	0.33	0.14	0.35
ZH	0.24	0.42	0.27	0.44
GL, SH, AR, AI, SG, GR, TG	0.11	0.32	0.11	0.32
LU, UR, SZ, OW, NW, ZG	0.07	0.26	0.07	0.26
TI	0.04	0.19	0.03	0.18
Number of Firms	3,533		3,533	
Number of Observation	130,976		468,328	

Notes: Columns 1 and 2 refer to the subsample of workers in jobs of lowest skill–level, columns 3 and 4 to the full sample of workers. Sources: All variables are taken from the SWSS, except the number of non–fatal accidents. Risk measure gives the number of non–fatal accidents per 1,000 workers per year, within cells over industry×skill–level. Own calculations, based on SWSS (2004) and SAIF (2004).

size or the geographic location of the employer).[5] Here, average monthly earnings are only about 70% of the overall average earnings (about 4,500 Swiss francs). Moreover, a worker from skill–level four is more likely to be a woman, more likely to be married and much more likely not to have Swiss citizenship, compared with a worker from the overall sample. The most striking difference between the overall sample and the lowest skill–level sample though is the distribution of workers with respect to educational attainment. As table 4.1 shows, there are practically no workers with an educational degree above vocational training. This, in fact, is a desired result with respect to the empirical approach we take (see section 4.4 below): Given that education (of course not exclusively) reflects differences in productivity, focusing on workers with similar educational attainment also implies that these workers are more similar with respect to unobserved productivity–relevant characteristics (compared to workers from all job skill–levels). We believe that the variance of unobserved productivity is presumably lowest within the group of workers in the lowest skill–level (although this presumption obviously is fundamentally empirically untestable).

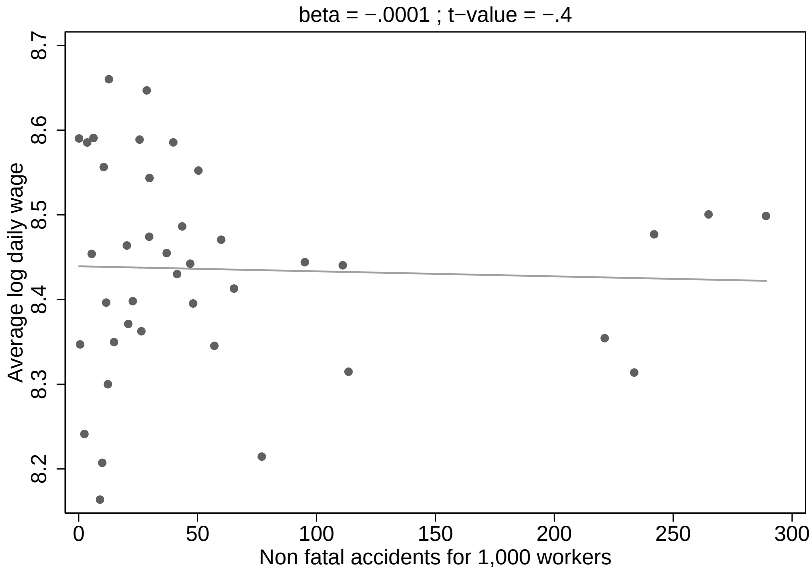
As table 4.1 also shows, the typical worker in the year 2004 was faced with the risk of a non–fatal, work–related accident of about 8.8% (88 accidents on average per 1,000 workers). In the sample of workers with lowest skill–level, the average risk was about half (about 43 accidents per 1,000 workers). Figure 4.1 shows a simple scatterplot between the average logarithmic monthly wage and the number of non–fatal accidents for workers from the lowest skill–level jobs at the level of industry$\times$skill–level. The scatterplot shows no relation whatsoever between the two variables (if anything, the correlation goes the "wrong" way), which is underlined by the estimated slope coefficient from a regression of the average log earnings on the number of accidents – yielding essentially a zero point estimate, both in economic and statistical terms (t–value is approximately zero). This result is not especially surprising though since average wages within industries clearly may not only reflect differences with respect to accident risks, but also differences in the composition of workers and jobs. We thus now move on to issues of identification and econometric estimation.

4.4 Identification and Estimation

We now discuss issues of identification and estimation of the compensating wage differential for (non–fatal) accident risk. We start with a simple hedonic wage regression of the

[5] The distribution of workers over the skill–level of jobs looks as follows: About 6% work in the highest level, about 20% in the second–highest level. 46% work in skill–level 3, and the remaining 28% of the workers are in jobs of lowest skill–level.

Figure 4.1: Log–Wage versus non–fatal injury risk, by industry



Notes: The y–axis shows the average logarithm of monthly gross earnings and the number of non–fatal accidents per 1,000 workers per year. Workers in lowest job skill–level only. Table A.1 in the chapter appendix shows the corresponding numbers. Own calculations, based on SWSS (2004) and SAIF (2004).

following form:

$$y_{ijk} = \alpha + x_i'\beta + z_j'\gamma + \delta r_k + u_{ijk} \tag{4.1}$$

Where y_{ijk} is the natural logarithm of the gross monthly wage of individual i, working in firm j and industry×skill–level cell k. x is a (column) vector of individual characteristics including citizenship, educational attainment, age (and its square), tenure (and its square), a gender–dummy and marital status. z is a (column) vector of characteristics describing the firm (and thus reflecting the characteristics of the job), and includes the size of the firm (and its square) and the geographical location of the firm. r is our risk measure, corresponding to the number of non–fatal accidents in industry×skill–level cell k per 1,000 workers in the year 2004. u_{ijk} is the unobserved error term, upon which identification of the compensating wage differential obviously critically hinges.

α, β, γ and δ are parameters to be estimated from the sample data at hand. The constant term α is, of course, of no special interest but simply serves the purpose of scaling the expected value of the error term to zero. The two parameter vectors β and γ are also, for the purpose of our analysis, of no particular interest. The parameter of main interest is δ, which, under appropriate assumptions, corresponds to the compensating wage differential for non–fatal accident risk.

As explained in section 4.3, the number of non–fatal accidents is only available for a single point in time, so that we can essentially only run a cross–sectional hedonic wage regression[6] (but we do have a partial panel structure with respect to wages, which we will try to capitalize on later; see section 4.4.4 below).

4.4.1 Unobserved Heterogeneity and Worker Sorting

Parameter δ (as are the other parameters) is identified if we are willing to assume that:

$$\mathbb{E}(u|x,z,r) = \mathbb{E}(u) \tag{4.2}$$

This means, if we can safely assume that the error term u_{ijk} is mean independent of (x, z, r), then all the parameters of the regression given by equation (4.1) are identified. However, as has been pointed out by several authors (e.g. Hwang *et al.*, 1992) and discussed in section 4.2, there is good reason to act on the assumption that there is unobserved individual heterogeneity related to wages (that is, these differences somehow reflect differences in productivity not taken into account for by observed variables) *and* that "safety" is a normal good (i.e. the demand for "safety" increases as income rises). Thus, workers of high productivity sort themselves into less risky jobs by accepting lower wages ceteris paribus. To stick with the model from equation (4.1), the hedonic wage regression with unobserved individual heterogeneity made explicit can be written as:

$$y_{ijk} = \alpha + x_i'\beta + z_j'\gamma + \delta r_k + \theta_i + \epsilon_{ijk} \tag{4.3}$$

where $(\theta_i + \epsilon_{ijk})$ corresponds to the error term u_{ijk} in equation (4.1) whereby now we make the problem of individual heterogeneity explicit (for simplicity, θ is rescaled such that the partial of effect of θ on y is equal to 1).[7] Now, even if we can assume that ϵ_{ijk} *is* mean independent of (x, z, r), identification of the compensating wage differential δ is only achieved if the unobserved effect θ is also mean independent of (x, z, r). Whenever there is reason to believe otherwise, parameter δ is not identified (and neither are the other parameters identified, but that is of minor importance for our purposes, since we are not per se interested in these parameters).

As discussed in section 4.2, the leading reason for a correlation between θ and the accident risk r is that θ reflects unobserved productivity, which is obviously related to the wage y. If the demand for safety actually increases with income and if we are, at

[6]Many, if not most, other empirical studies face the same problem of not observing the relevant risk measure over time, as pointed out by Hwang *et al.*: "While studies of this sort [i.e. panel studies] represent improvements over standard cross–sectional studies, their applicability is restricted by the availability of longitudinal data sets that include the relevant nonwage job attribute variables. In most cases, this is a binding constraint." (Hwang *et al.*, 1992, p. 836).

[7]Note that the error term ϵ_{ijk} potentially also includes unobserved heterogeneity with respect to the firm. We will take up this issue in section 4.4.4 below.

the same time, unable to adequately control for productivity differences, then this could quite plausibly lead to a correlation between θ and r. That is, more productive workers (i.e. workers with above–average θ) sort themselves into less–risky jobs by accepting lower wages, which in turn leads to a correlation between the productivity measure θ and the risk measure r, meaning that identification of the risk compensation parameter δ must ultimately fail.

In the following, we will discuss three different empirical approaches in turn, all of which are intended to mitigate the worker–sorting leading to biased estimates of the compensation for risk.

4.4.2 Control Function

The first approach, which we might label control–function approach, is to basically stick with the hedonic wage regression, but to try to control for as many observable characteristics (both at the individual and the firm/job level) as possible. In fact, controlling for the appropriate set of observed variables might entail identification of δ, depending on which variables are observed, and thus can be controlled for in the regression model. Under 'typical' circumstances however, this approach is prone to fail identification, since the data sources usually available do not include enough control variables or the critical control variables, respectively. Nonetheless, we will also estimate hedonic wage regressions, mainly for reasons of comparison. We stress here that we would not place much confidence in the resulting estimates for the parameter δ. The bottom line is that this approach to identification crucially hinges on the availability of enough control variables (describing both the workers and the jobs).

4.4.3 Sample Stratification

A second related approach is to stratify the sample in such a way as to minimize the variation in the unobserved error component θ (see equation (4.3)). That is, we run the very same hedonic wage regression as given by equation (4.1), but only on a narrow subset of individuals. Ideally, this subset consists of individuals presumably as similar as possible with respect to θ. That is, stratification is the simple non–parametric counterpart of the control function approach. However, since most often it is very difficult to control for θ, we think that stratifying the sample is probably a more fruitful approach.

Our stratification variable of primary interest is the skill–level of the job, which is recorded in the SWSS. Let $s_{ij} \in \{1, 2, 3, 4\}$ be the skill–level of individual i working in job j, where $s = 1$ ($s = 4$) corresponds to the highest (lowest) skill–level of a given job. We thus run the same hedonic wage regression as in equation (4.1), but only on a subset of individuals within a given skill–level s. Specifically, we will run the following regressions:

$$y_{ijk} = \alpha + x_i'\beta + z_j'\gamma + \delta r_k + u_{ijk} \quad s_{ij} \geq s \in \{1,2,3,4\} \tag{4.4}$$

Note that this approach to estimation is basically the same as the control function approach, the main difference being that stratification allows *all* parameter estimates to vary between different subsets of the sample[8]. However, we think it plausible that the main advantage of the stratification is that we can minimize variation in θ in this way, which ideally renders a consistent estimate of the compensating wage differential δ.[9]

4.4.4 Wage Decomposition and Firm Wage–Component

Our third approach to identification and estimation is based on quite another idea, which tries to capitalize on the availability of panel data (with respect to the firm).[10] Still, we can use the additional source of variation in wages stemming from the fact that the SWSS has a longitudinal structure (at least with respect to the firm) such that we can apply simple panel data methods (see, for example, Wooldridge, 2002).

To start with, let us assume that the observed natural logarithm of the wage y_{it} of individual i in a given year t can (conceptually) be decomposed in a linear model as follows:

$$y_{ijt} = \lambda_t + \phi_i + \psi_j + \epsilon_{ijt} \tag{4.5}$$

Abstracting from the time fixed–effect λ_t, equation (4.5) states that individual i's wage is the sum of an individual wage fixed–effect ϕ_i, a firm wage fixed–effect ψ_j, and a remaining random error component ϵ_{ijt}. The critical assumptions in this simple linear fixed effects model are the assumptions about the time invariance of both the individual and the firm fixed effect. However, since we are using panel data spanning only a short time period we believe that these assumptions are innocuous for our application – nonetheless allowing us to resort to the power of panel data methods. Importantly, note that the theory of

[8]That is, the control function approach yields the same estimates as sample stratification if all parameters would be interacted with the variable on which stratification is based on. However, such a fully interacted regression model is, due to the large number of parameters to be estimated, often difficult to interpret.

[9]As we will show later, our stratification approach actually reduces the differences between groups of workers with respect to the observed wage (on this point, see table 4.5). For example, in the overall sample the difference in mean monthly earnings between men and women amounts to about 1,700 Swiss francs (about one third relative to the female average). In the subsample of workers within the lowest skill–level, the difference in average earnings amounts to only about 630 Swiss francs (relative to the female average, a bit less than 15%). Although this is only suggestive evidence, we still believe that this exactly what one would expect if the presumption holds that the variance in θ is lower in the lower skill–levels of jobs.

[10]Of course, we could capitalize on repeated individual observations using for example the techniques proposed by Abowd and Kramarz (1999), but as explained in section 4.3, we only have temporal information about the employer but not the individual workers.

compensating wage differentials essentially makes statements about the wage component specific to the employer (i.e. ψ_j), but not to the individual–specific part nor the random part of the wage.

This simple representation of the wage essentially states that the wage of a specific individual i in a given year t is the sum of an aggregate time effect (e.g. aggregate shocks), an individual–specific component (which is assumed to be time–invariant), a firm–specific part (also assumed to be time–invariant) and a random error term (varying over time, firms, and individuals). If it is possible to consistently estimate the wage firm fixed effect ψ_j from the available data, we can essentially get rid of individual heterogeneity by simply running a hedonic wage regressing using the estimated wage firm–fixed effect $\hat{\psi}_j$ instead of the observed wage y_{ijt} on our risk measure r, although we can not directly control for unobserved individual heterogeneity in the hedonic wage regression (because, remember, the risk measure is *not* observed over time and because there is no person–identifier in the SWSS).

Thus, in a first stage, we run a simple regression model using the three consecutive waves of the SWSS:

$$y_{ijt} = \alpha + x'_{it}\beta + z'_{jt}\gamma + \lambda_t + \psi_j + u_{ijt} \quad \text{with} \quad s_{ij} = 4 \tag{4.6}$$

Here, again, x and z are vectors of observed individual and firm characteristics and the parameter λ_t captures aggregate wage shifts over time. The vector x of observed individual characteristics is important here because we essentially use x to proxy for otherwise unobserved individual heterogeneity. Moreover, we run this regression on a subset of individuals working in jobs with the lowest skill–level only, such that we can further dampen the problem of unobserved heterogeneity.

The regression model given by equation (4.6) is only of interest here because it allows us to estimate the firm wage fixed effects, represented by the vector ψ_j. Practically, ψ_j is estimated from the data by including a separate dummy variable for each firm in the sample.

In the second stage, we run a regression very similar to the hedonic model from equation (4.1):

$$\hat{\psi}_{ijk} = \alpha + x'_i\beta + z'_j\gamma + r_k\delta + u_{ijk} \quad \text{with} \quad s_{ij} = 4 \tag{4.7}$$

where now the dependent variable is the estimated firm wage fixed effect $\hat{\psi}_{ijk}$ of individual i working in firm j. Note that the unit of observation is still the individual worker, although the firm fixed effect obviously does not vary between individuals working in the same firm. This procedure, though, directly applies the right weighting scheme. Again, r_k is the non–fatal risk measure in industry×skill–level cell k. Note that we still have to include both vector x and z, because the estimated wage firm fixed effect $\hat{\psi}$ is not independent of x and z. The main point is that the estimated wage firm fixed effect $\hat{\psi}$ should have been separated from the unobserved individual–specific component θ.

4.5 Econometric Results

We now present the econometric results, starting with some simple hedonic wage regressions. We then go to discuss the results from stratifying the sample by skill–level, which yields results in the expected direction. Next, we present results from our preferred approach, regressing firm wage fixed–effects instead of individual wage on accident risk. Finally, we present empirical estimates for the statistical value of a injury (i.e. a non–fatal accident related to workplace activities), which are implicitly given by the estimates of the different econometric models.

4.5.1 Hedonic Wage Regression

Estimated parameters of the hedonic wage function, as given by equation (4.1), are given in table 4.2 (column 1). The point estimate of the non–fatal accident risk is negative (-0.00005), although statistically not different from zero (t–value of about less than one in absolute value). This result is in fact in line with either endogenous sorting of workers. Note also that the other regressors have the expected sign. As discussed in section 4.4, the leading explanation for the "wrong" sign of the risk variable is endogenous sorting of workers into jobs with different risks. As we do not put much confidence in this simple hedonic wage regression, so we quickly move on to the next results.

4.5.2 Sample Stratification

Columns 2 to 4 in table 4.2 also show parameter estimates from a simple hedonic wage regression, but only for a subset of workers each. As we narrow the range of the skill–level, the point estimate of risk compensation moves towards the expected direction. Focusing on workers in the lowest skill–level only yields a positive point estimate on the risk measure (0.00024), which moreover is almost statistically significant on the 10% level (t–value of 1.63). The decrease in the R–squared of the model reflects the fact that the stratification of the sample absorbs a large part of the variation in the regressors (e.g. educational attainment; see section 4.3), which otherwise explain a significant part of the variation in wages.

4.5.3 Wage Firm Fixed Effects

The last column in table A.1 in the chapter appendix shows the estimated firm wage fixed–effect by industry (at the two–digit level, only for the lowest skill–level of jobs). As

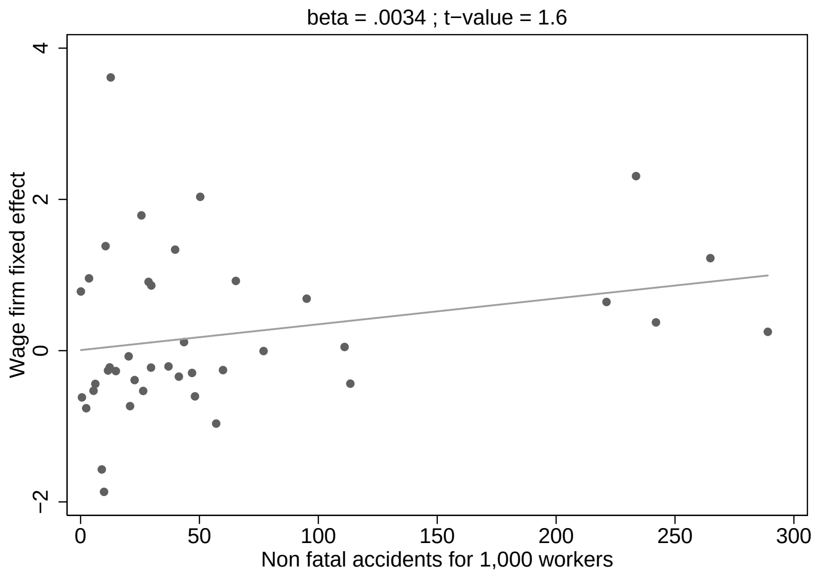
Table 4.2: Hedonic wage regressions, by skill–level of job

	ln(monthly wage) (y)			
Skill–level(s) of job	1–4	2–4	3–4	4
Non fatal accident risk	−0.00005	−0.00003	0.00001	0.00024
	(−0.88)	(−0.60)	(0.30)	(1.63)
(Plant size / 100)	0.00193**	0.00179**	0.00175**	0.00148*
	(2.92)	(2.98)	(2.77)	(2.16)
(Plant size / 100) squared	−0.00001**	−0.00001***	−0.00001**	−0.00000*
	(−3.49)	(−3.60)	(−3.31)	(−2.12)
Age	0.03299***	0.03329***	0.03028***	0.01733***
	(17.36)	(17.10)	(15.80)	(9.61)
Age squared	−0.00034***	−0.00035***	−0.00032***	−0.00019***
	(−18.23)	(−17.70)	(−16.27)	(−9.18)
(Tenure / 10)	0.07161***	0.07189***	0.07910***	0.11261***
	(5.06)	(5.13)	(5.63)	(6.65)
(Tenure / 10) squared	−0.00834*	−0.00807*	−0.00873*	−0.01767***
	(−2.40)	(−2.37)	(−2.64)	(−4.19)
Constant	8.91118***	8.70657***	8.59138***	8.67286***
	(121.62)	(119.71)	(120.24)	(50.83)
n	468, 328	441, 269	346, 916	130, 976
R^2	0.623	0.556	0.459	0.321

Notes: *, **, *** denotes statistical significance on the 5%, 1% and 0.1% level, respectively. Robust t-values in parentheses. Skill–level 1 (4) corresponds to the highest (lowest) skill–level possible.

shown in figure 4.2, a simple scatterplot of the average firm wage fixed–effect (averaged within industries) versus the number of non–fatal accidents now shows a clear positive relation between the two variables (as opposed to figure 4.1, which showed no relation between the two measures at all). A simple regression of the average wage firm fixed effect on the number of non–fatal accidents yields an estimated slope coefficient of 0.0034, which marginally reaches statistical significance (t–value of about 1.6). Column 1 of table

Figure 4.2: Firm fixed effect versus non–fatal injury risk, by industry



Notes: The y–axis shows the average of the wage firm fixed effect and the x–axis the number of non–fatal accidents per 1,000 workers per year. Workers in lowest job skill–level only. Also see table A.1 in the chapter appendix. Own calculations, based on SWSS (2004) and SAIF (2004).

4.3 reproduces, for the purpose of comparison, the simple hedonic wage regression using workers from the lowest skill-level only (see section 4.5.1 above). As it turns out (see column 2, table 4.3), the point estimate of the risk parameter more then doubles when using $\hat{\psi}$ instead of y directly as the dependent variable in the regression, yielding a point estimate of 0.00067 (with a t–value of more than 2). This result is in line with the story of workers sorting into jobs based on their (partially) unobserved productivity, because the main difference between columns 1 and 2 of table 4.3 is that variation in y still reflects to a large part variation in unobserved productivity, whereas variation in $\hat{\psi}$ much less so.

4.5.4 Detailed Results

We present some additional results for different subgroups of the sample, based on both the simple hedonic wage model and on models using the wage firm fixed effect as the dependent variable. The estimates of these additional models are given in table 4.4.

Table 4.3: Observed wage versus wage firm fixed effect (skill–level 4 only)

	ln(monthly wage)	
	Observed wage (y)	Firm fixed effect ($\hat{\psi}$)
Non fatal accident risk	0.00024	0.00067*
	(1.63)	(2.41)
(Plant size / 100)	0.00148*	0.00189*
	(2.16)	(2.10)
(Plant size / 100) squared	−0.00000*	−0.00000
	(−2.12)	(−1.63)
Age	0.01733***	0.00437*
	(9.61)	(2.49)
Age squared	−0.00019***	−0.00005*
	(−9.18)	(−2.68)
(Tenure / 10)	0.11261***	0.03799**
	(6.65)	(2.80)
(Tenure / 10) squared	−0.01767***	−0.00793*
	(−4.19)	(−2.42)
Constant	8.67286***	−0.08190
	(50.83)	(−1.31)
n	130,976	130,976
R^2	0.321	0.201

Notes: *, **, *** denotes statistical significance on the 5%, 1% and 0.1% level, respectively. Robust t-values in parentheses. Only workers in jobs of lowest skill–level. Own calculations, based on SWSS (2004) and SAIF (2004).

These additional estimates are consistent with our main result, since in each case the model using the wage firm fixed effect as the dependent variable yields a higher risk compensation than using the observed wage. Panel A of table 4.4 simply reproduces the result from table 4.3 discussed above for easy comparison with the other results.

Additionally, these estimates may shed some light on the question of the sorting of workers into firms with different risk compensation and possibly on differences in risk aversion between groups of workers.[11] Note that, by construction, the estimated firm wage fixed effect $\hat{\psi}_{ijk}$ is the same for all individuals working within a specific firm j. It thus must be the case that differences in the estimated risk compensation between subgroups of workers somehow reflect differences in risk compensation between firms. We will be more explicit on this point below when discussing the results.

First, we split the sample by gender (panel B of table 4.4). The hedonic wage model gives positive point estimates for both men and women, although both are not statistically different from zero. Interestingly, the point estimate of the compensating wage differential

[11] We also split the sample by marital status (i.e. married versus single individuals). We did not find (statistically) different results and we thus do not present these results.

is larger for women ($\hat{\delta} = 0.00046$) than for men ($\hat{\delta} = 0.00015$). Using the wage firm fixed effect yields, in both cases, a higher point estimate than using the observed wage ($\hat{\delta} = 0.00038$ for men, and $\hat{\delta} = 0.0015$ for women), but now in this case both coefficients are statistically different from zero. Still, the estimate for women remains about three times as large as the corresponding estimate for men.

We believe that such a pattern is informative with respect to the underlying sorting of workers into firms with different risk compensation. The results essentially state that women ask a higher risk compensation than men *for a given* change in the statistical non–fatal accident risk. This result is in line with empirical evidence on differences in risk aversion between men and women (Sunden and Surette, 1998).

Second, table 4.4 (panel C) also shows separate results for smaller (that is, less than 500 employees) and larger (500 or more employees) firms. The simple hedonic wage regression for smaller firms gives us a positive and significant point estimate for risk compensation ($\hat{\delta}$=0.00031, t–value of about 3.3). For larger firms, we find no effect of accident risk on the firm wage fixed effect (the point estimate is even negative). Moving on to the fixed

Table 4.4: Wage firm fixed effects, detailed results

	Log–Wage	Firm fixed effect
A. Overall sample	0.00024	0.00067*
	(1.63)	(2.41)
n	130,976	130,976
R^2	0.321	0.201
B. By gender		
Men	0.00015	0.00038***
	(0.96)	(3.81)
n	60,219	60,219
R^2	0.304	0.192
Women	0.00046	0.00150*
	(1.34)	(2.61)
n	70,757	70,757
R^2	0.240	0.270
C. By size of firm		
Smaller firms	0.00031**	0.00070**
	(3.29)	(3.36)
n	75,911	75,911
R^2	0.293	0.185
Larger firms	−0.00001	0.00055
	(−0.02)	(1.09)
n	55,065	55,065
R^2	0.395	0.243

Notes: *, **, *** denotes statistical significance on the 5%, 1% and 0.1% level, respectively. Robust t-values in parentheses. Own calculations, based on SWSS (2004) and SAIF (2004).

effects regression, we again get a larger point estimate for the smaller firms ($\hat{\delta} = 0.0007$, t–value of about 3.4) and larger firms ($\hat{\delta} = 0.00055$), although for larger firms the estimate remains statistically insignificant.

This result states that smaller firms have to pay higher risk compensation for any increase in the risk of non–fatal accident than larger firms do. This difference in risk compensation might reflect underlying differences in the wage setting process between firms of different size. Specifically, one might argue that wages in smaller firms are more likely to reflect competitive wages than in larger firms, where rent sharing is presumably more prevalent than in smaller firms. Another possible explanation for this finding is that workers may perceive working at larger firms per se as more safe (for whatever reason). In statistical terms, in fact, larger firms do not pay any risk compensation at all, which possibly means that larger firms have to guarantee workplace safety anyway because they are presumably under stricter monitoring, whereas smaller firms have more discretion with respect to workplace safety and thus to risk compensation.

4.5.5 The Value of a Statistical Injury

Given an estimate for the compensation for non–fatal accident risk, we can easily compute the value of a statistical injury (i.e. non–fatal accident). Because all our estimates of the risk parameter are based on semi–logarithmic regressions, the estimated risk coefficient corresponds to the *relative* wage which 1,000 workers are willing to forego in order to prevent one non–fatal accident (and thus is independent of the time period chosen). Thus, multiplying the estimated risk parameter by 1,000 yields the estimated *relative* value of a statistical injury (VSI):

$$\text{VSI} = \hat{\delta} \cdot 1,000 \tag{4.8}$$

Since our risk measure refers to non–fatal accident per year, we will phrase the VSI in terms of average annual earnings (that is, we multiply VSI additionally with the average annual earnings in the corresponding group of workers). Table 4.5 shows estimates for the value of a statistical injury computed from the different estimation methods discussed above (given in terms of the average annual earnings in the sample). The main estimates are based on the point estimate of the risk variable. Lower and upper bounds on the value of a statistical injury are based on the 95% confidence interval of each point estimate of the parameter δ. The simple hedonic wage regression actually yields a negative estimate for the value of a statistical injury (per injury per year). Only using the upper bound of the confidence interval yields the expected positive value (although still small).

Stratification of the sample yields a higher value of a statistical injury, the narrower the sample. Focusing on workers in the lowest skill–level only gives an estimate of about 14,000 Swiss francs (the estimate based on the lower bound of the confidence interval

Table 4.5: The estimated value of a statistical injury

	Yearly earnings	Estimated value of a statistical injury (VSI), based on		
		Lower bound of $\hat{\delta}$	Point estimate of $\hat{\delta}$	Upper bound of $\hat{\delta}$
A. Hedonic wage function				
Skill–level 1–4	76,464	−12,512	−3,823	4,866
B. Stratification				
Skill–level 2–4	71,496	−9,294	−2,145	5,005
Skill–level 3–4	64,572	−3,659	646	4,951
Skill–level 4 only	54,324	−2,959	13,038	29,035
Men	58,380	−9,487	8,757	27,001
Women	50,856	−11,522	23,394	58,310
Smaller firms	54,120	6,578	16,777	26,976
Larger firms	54,588	−55,134	−546	54,042
C1. Wage firm fixed effect				
Skill–level 4 only	54,324	6,192	36,397	66,602
C2. Wage firm fixed effect: Subsamples				
Men	58,380	10,539	22,184	33,830
Women	50,856	17,829	76,284	134,739
Smaller firms	54,120	15,334	37,884	60,434
Larger firms	54,588	−25,065	30,023	85,112

Notes: All entries are based on the point estimate, the lower and upper bound of the 95% confidence interval of $\hat{\delta}$, respectively. Own calculations, based on SWSS and SIAF.

though still gives a negative estimate).

Using the wage firm fixed effect finally gives a consistent positive value of a statistical injury (even if we use the lower bound of the corresponding confidence interval). Using the point estimate, we get an estimated value of a statistical injury of about 40,000 Swiss francs per non–fatal accident averted per year. This value fits into the range reported by most other studies (see Viscusi and Aldy, 2003, again).

4.6 Conclusions

We provide empirical estimates of the value of a statistical injury for Switzerland for the year 2004, using non–fatal accident risk within industry×skill–level cells and applying different approaches to identification. Specifically, we try to statistically isolate the firm–specific wage component, to which the theory of compensating wage differentials conceptually applies most directly. Further, we try to mitigate the problem of endogenous worker sorting as far as possible by combining appropriate data and methods.

The empirical method actually makes a huge difference with respect to the estimation of risk compensation. Simple hedonic wage regressions actually yield negative or zero compensation for non–fatal accident risk at the workplace. Moving on to methods we believe are more reliable (i.e. consistent) pushes the risk compensation in the "right" direction (i.e. yielding positive compensation for accident risk). Our preferred estimation method, based on a restricted sample of workers in jobs of lowest skill–level only and using the wage firm fixed effect instead of the observed wage, gives an estimate for the value of a statistical injury of about 40,000 Swiss francs, which is within the range given by both studies from inside and outside the U.S. labor market.

Our analysis, by comparing the magnitude of risk compensation, may also shed some light on the problem of endogenous sorting of workers based on their (unobserved) productivity–relevant characteristics. The more attention we pay to mitigating unobserved productivity differences, the larger the estimates for risk compensation we get. This pattern seems to be consistent with the hypothesis that high–productivity workers select into lower–risk jobs by accepting lower wages.

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4.A Appendix

Table A.1: Main variables, by industry (lowest skill–level only)

Industry	Workers	Earnings	Accidents	FFE
Petroleum refining and processing	4692	5,560.13	0.14	0.78
Office material production, data processing	4288	4,302.83	0.59	-0.62
Information technology services	6237	3,933.30	2.39	-0.76
Shipping	55	5,467.47	3.57	0.96
Metal production and processing	7201	4,781.81	5.46	-0.53
Aviation	12	5,496.25	6.22	-0.44
Production of leather goods and shoes	229	3,628.01	8.94	-1.57
Production of clothes and fur goods	270	3,741.27	9.89	-1.87
Insurance industry	2086	5,300.57	10.53	1.38
Production of medical technology	7421	4,523.07	11.55	-0.26
Retail business	19118	4,090.10	12.26	-0.22
Tobacco processing	636	5,977.87	12.70	3.61
Production of furniture, jewellery, musical intruments	1743	4,329.91	14.86	-0.27
Machinery/mechanical engineering	5441	4,851.64	20.24	-0.07
Textiles	1350	4,436.00	20.83	-0.73
Automobile industry	1075	4,508.15	22.73	-0.39
Energy– and watersupply	496	5,504.46	25.59	1.79
Traffic support	1502	4,360.78	26.35	-0.53
Credit business	3059	5,833.48	28.60	0.91
Paper and carton production	2153	4,917.06	29.64	-0.22
Credit business and insurance industry	70	5,373.94	29.76	0.86
Printing, publishing and distribution industries	3013	4,833.14	36.99	-0.21
Research and development	202	5,478.94	39.78	1.34
Whole sale	7621	4,683.02	41.36	-0.34
Wood processing	810	4,950.09	43.53	0.11
Transportation	2236	4,724.08	46.89	-0.29
Rubber and plastic production	2657	4,511.65	48.12	-0.60
Mining	80	5,277.08	50.33	2.03
Agriculture	6756	4,310.73	57.05	-0.96
Mining	1217	4,821.76	59.91	-0.26
Health and welfare system	19642	4,582.02	65.31	0.92
Hotel and restaurant industry	9676	3,743.90	76.98	-0.01
Real estate	581	4,784.07	95.10	0.69
Information transmission	55	4,707.71	111.04	0.05
Entertainment	814	4,208.07	113.46	-0.44
Education	744	4,394.47	221.19	0.64
Personal services	238	4,318.43	233.62	2.31
Waste management	95	4,953.19	242.00	0.37
Lobby, associations, organizations	512	5,067.35	264.88	1.22
Construction	4893	4,965.64	289.03	0.25

Notes: Table entries show sample averages within industries. Non–fatal accident risk is the number of non–fatal accidents per 1,000 workers. Wage is the average logarithm of gross monthly earnings. Wage firm fixed effect is the average firm fixed effect, as given by equation (4.6), and is (in the table) standardized to mean 0 and variance 1. Own calculations, based on SWSS and SIAF.

CHAPTER 5

Closing Words

"La statistique: une personne complaisante qui ne refuse rien de ce qu'on lui demande habilement."

Edouard Herriot (1872-1957), French politician

As pointed out by Edouard Herriot, statistics is like a compliant person who refuses nothing that is cleverly requested. Nevertheless, the explanatory power of statistics is limited by the available data. The data preparation was not only the most time-consuming part of this thesis but also the cornerstone of the entire analysis. Chapter 2 and 3 are based on Austrian data, whereas chapter 4 uses Swiss data.

Chapter 2 deals solely with the Austrian Social Security Database (ASSD), a large administrative longitudinal data set. The data has been kindly provided by the "Hauptverband der österreichischen Sozialversicherungsträger". The data was originally collected to calculate old age social security benefits and covers the universe of all private sector workers from 1972 to 2002. The unusually long observation period has allowed me to analyze long term earnings losses of workers displaced due to a plant closure at two different points in the business cycle. I could furthermore separate workers displaced from small firms from workers displaced from large firms, because workers and firms are linked in the ASSD. For this chapter the data preparation was of crucial importance. All of the results are dependent on how one defines a plant closure firm. Therefore, I have attached great importance to a very accurate definition of a firm going bankrupt. This is essential in order to exclude as many differences between boom and recession as possible. I was able to shed new light on a very special, but in my opinion very interesting, puzzle in the literature on earnings losses of displaced workers. Results indicate that workers displaced from a small firm in a economic situation which is getting better and better are somehow labeled as less productive and receive lower wages than workers displaced from small firms during a recession or those displaced from large firms no matter what the economic climate.

Chapter 3 complements the ASSD with data from the Austrian statutory accident insurance, reporting the complete history of workplace accidents from 2000 to 2002. This data has been kindly provided by the "Österreichische Allgemeine Unfallversicherungsanstalt" (AUVA). The two data sets can be linked via the individuals' anonymous social security number. This allows us not only to split observed wages into a firm-wage component and a worker wage component, but also to split observed injuries into a part attached to the worker and a part specific to the firm. In doing so we can identify the building block of the theory by regressing the firm wage component on the firm risk component. Our results show that high productive workers do not sort themselves into low risk jobs, therefore we can show that the bias obtained from standard cross-section hedonic wage regressions is rather small. But we do find that conditional on firm risk, high risk workers sort themselves into high risk jobs.

Chapter 4 uses data from two sources. First, the "Schweizerische Unfallversicherungs Anstalt" (Suva) provided us with the number of non–fatal accidents within industry×skill–level cells. Second, the "Bundesamt für Statistik" (BfS) provided us with data including individual monthly earnings and several individual–specific and firm–specific characteristics. The advantage of the Swiss data is that accidents are recorded separately for four different skill levels of workers, which allowed us to focus on workers in the lowest skill level. A further advantage is that the data includes, beside many other individual specific characteristics, the educational level of all workers. The preferred point estimate uses in this chapter for the compensating wage differential corresponds to a value of a statistical accident of 40,000 Swiss francs. Further we were able to show that women demand a higher risk compensation than men for a given change in the statistical non fatal accident risk.

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